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THE CLASSIFICATION OF NERVOUS REACTIONS[1]

IT is within the memory of most of us what a distinct advance was made in the definiteness of our thinking about nervous reactions when the introduction and improvement of the Golgi method led up to the conception of the neurone doctrine. Previous to that time our mental picture of the reflex mechanism was not essentially incorrect; but its conception of the nature of the connection between the sensory fiber and the motor nerve cell was indefinite. When the new histological method revealed the posterior root fibers entering the cord and by means of collaterals ending in the immediate neighborhood of motor cells, there was revealed an almost diagrammatic mechanism which explained many reflex phenomena; and we can recall the enthusiasm with which all proceeded to construct combinations of neurones to serve as the anatomical basis of the various known functions of the nervous system; indeed, we have been engaged in this fascinating pastime ever since.

This is exactly as it should be, for only in this way could the possibilities of the new discovery be tried out. There is danger, however, in anything which is attractively definite; sometimes because it may belong among those things which are "too good to be true"; but more frequently because its successful explanation of many of the phenomena with which it deals may blind us to its failure to explain others;

[1] Address of the chairman and vice-president of Section K (Physiology and Experimental Medicine), American Association for the Advancement of Science, Philadelphia, December 31, 1914.

and I think there are to-day, even among those who do not follow Apathy and Bethe in their undiscriminating attacks on the neurone theory, many who are seriously asking whether the neurone conception of the reflex exhausts the possibilities or nervous mechanisms.

For, after all, the distinct service of the neurone theory is its explanation of the mechanism of reflex action. It gives us a satisfactory explanation of simple and even of highly coordinated reflexes; but there are still left problems upon which its staunchest defenders will not claim that it throws much light. The contribution, for example, of the cerebellum to the execution of volitional or reflex actions is not self-evident in terms of the theory; the mechanisms of reenforcement and facilitation (Bahnung) are no more easily pictured now than before; and, above all, the whole field of cerebral physiology may be said still to be in the same state as was that of reflex action before Golgi made that fortunate mistake of putting some pieces of spinal cord which had been hardened in Müller's fluid into silver nitrate and beheld for the first time a nerve cell in all its glory.

The diagrammatic clearness of the picture of the reflex mechanism thus revealed has contributed largely to our present mental approach to the problems of neurology, an approach which is faithfully reflected in our text-book presentations of the subject. The first text-book chapter is largely anatomical, chiefly histological; the next chapter deals with reflex action which is presented to the student as par excellence the typical nervous action; the treatment of the subject then proceeds from the simpler reflexes to those requiring a higher degree of coordination; accustomed movements, such as those of locomotion, receive their explanation as an endless chain of reflexes requiring for its operation the structures of the mid- and 'tween brains; finally the attempt to explain everything in terms of reflex action is carried into the field of cerebral physiology. With what success? One has only to read the text-books to find out. Some things, especially localization, are dwelt upon at length; the possibilities of excessively complex coordinations are suggested by the anatomical structure; but we miss entirely the satisfaction of seeing the cerebral functions clearly pictured in terms of neurone structure. We trace the "way in" and the "way out"; we see that the connection between the afferent and efferent nerve fibers is in the cortex; but what takes place in the cortex? Is it objectively nothing more than our typical reflex raised to the *n*th power of complexity? Perhaps it is; but does any one feel reasonably sure of it? For one, I confess I do not.

However that may be—and I have no intention of discussing the question—this much may certainly be said. We know that there are nervous actions which are not reflexes at all; furthermore, there are nervous actions which usually pass as reflexes, although they present striking and perhaps fundamental points of difference from the typical reflex arc of our neurone theory. The justification of these statements will be attempted in what follows. My present purpose and indeed the purpose of this paper is to challenge the wisdom of making the reflex arc the type of all nervous action either in our own thinking or in the presentation of the subject to students, and to suggest that we would act more wisely to cultivate a more open state of mind with regard to the existence of other possibilities.

This may be done, it seems to me, by drawing sharply the distinction at the outset between the following classes of nervous

action: automatic, axon reflex, unconditioned reflex, conditioned reflex and volitional. It is not claimed that this list is exhaustive. The physiology of such mechanisms, for example, as the plexus of Auerbach is at present too little understood to admit of successful classification. It is only claimed that the above are distinct forms of nervous activity; that they are carried out by different mechanisms, and that, as such, they should be given coordinate rank in the student's mind.

In the following discussion I shall not include any treatment of volitional action. I am concerned only with the proper classification of nervous actions and see no reason for changing the all but universal custom of placing volitional actions in a class by themselves. I shall, however, dwell at some length upon the automatic action, the axon reflex, and then discuss together the conditioned and the unconditioned reflex.

AUTOMATIC ACTIONS

In general an automatic action is one which originates in the mechanism involved and is not caused by any external influence acting only at the time of its occurrence. The ticking of a clock is an example. In the field of physiology we think at once of the beat of the heart, although other no less striking examples are known; a strip of the muscular coat of the stomach or intestine shows automatic contractions; and many of the processes of embryological development probably belong in the same category. Contrasted with these are skeletal muscles and many glands which become functionally active only in response to some sort of external stimulus, usually a nerve impulse.

Even in the case of the skeletal muscle, however, the external stimulus seems to act by causing the accumulation, and probably the localized accumulation of some physical or chemical condition within the cell leading to the discharge of energy. Thus an attractive theory of electrical stimulation of skeletal muscle supposes that certain semipermeable membranes within the muscle fiber are more permeable to ions of one electrical charge than to those of the opposite charge; hence in the migration of the charges to the two electrodes during the passage of a current there results an accumulation of electric charges at these membranes; and when this has gone on to a certain extent, the electrical condition thus created explodes an unstable fuel substance, energy is liberated and contraction results. The passage of the stimulating current has merely produced the accumulation of what may be called the "discharging conditions" within the cell.

In the automatic action these discharging conditions seem to accumulate without external assistance, possibly as the result of certain metabolic processes in the cell itself. External conditions, such as temperature, may influence the rate or the amount of discharge; but this does not make these external conditions stimuli in any true sense. Furthermore, we may speak, if we will, of the whole chain of events leading to the accumulation of the discharging conditions as an "inner stimulus"; but this would seem to involve an unnecessary and even questionable extension of the term stimulus.

An automatic nervous action is frequently defined as a discharge from a nerve cell caused by some other external stimulus than that of an exciting neurone; but, if the cell is discharged by an external stimulus of any kind, the action is not automatic. What we observe in such cases is activity, apparently, at any rate, arising within the cell itself, and we have no more

logical right to assume an unrecognized stimulus than in the case of the ticking of a clock. In the absence of knowledge to the contrary, the presumption is that the cell is discharged by the operation of its own never ceasing metabolism.

In our usual teaching as well as in our usual thinking it is customary to take account only of (1) the stimulus and (2) the reaction, and to regard each of these as a single process; whereas all the evidence goes to show that between the ordinary external stimulus, at any rate, and the release of energy there is usually interpolated a third process, which we have termed the "accumulation of discharging conditions" in our brief reference to the nature of electrical stimulation. We again meet with the suggestion of a similar process in the case of stimulation by a nerve impulse. Langley's work on the antagonism of nicotin and curare, as well as that of Keith Lucas on the "characteristic" of stimulation in different tissues, has led to the assumption of a "receptive substance" in skeletal muscle. The action of adrenalin also points to a similar "receptive substance" connected with the endings of the post-ganglionic automatic neurones. And yet is not this term "receptive substance" or "receptor" merely a name to hide our ignorance? and do we not really mean a physical or chemical process carried out by the cell protoplasm, as a whole, rather than a specialized irritable substance; a process, in other words, which results in the accumulation of "discharging conditions"? If this point of view is correct we must distinguish sharply between the stimulated action and the automatic action; they are alike in the second and third of the above-mentioned processes; they differ in that the accumulation of discharging conditions comes in the stimulated action as the result of an external influence (electric shock, nerve impulse, or mechanical blow), while in the automatic action it results presumably from the cell metabolism.

In the central nervous system the best known and most successfully studied case of automatism is that of the respiratory center. The conclusion which Rosenthal drew from his experiments, that the nerve cells of this center send out rhythmic discharges when removed from all connection with afferent nerves, has been confirmed by all subsequent work, the experiments of Winterstein being especially conclusive on this point. Here again we are probably dealing with the development within the nerve cell of discharging conditions which may be influenced by the character of the environment, such as the tension of carbon dioxid or the concentration of hydrogen ions, or temperature or the presence of certain drugs; and we may repeat that there is no justification for speaking of these as stimuli, as we generally do. So far as the facts go, we may logically regard them only as external conditions which regulate the rate of development of the automatic cell processes or the character of the discharge which it evokes from the cell.

Until recently no other case of automatic nervous action was known. Some may have been suspected in the vaso-motor system or in the myenteric plexus; but no facts compelled the conclusion that they must be regarded as automatic actions. Recently, however, facts have come to light which argue strongly for an automatic basis to the nervous mechanism of locomotion. These movements, as already stated, have in the past presented to us the picture of an endless chain of reflexes, in which the afferent neurones are mostly the nerves of muscular sense, the complex of instreaming afferent impulses, ever changing as the movement proceeds, giving the

appropriate stimuli to the successive movements, which eventually come back to the starting point and so lead to the repetition of the series. To avoid misunderstanding, it may be well to say at the outset that no one denies that afferent impulses play an important rôle in locomotion. The phenomena of locomotor ataxia are conclusive evidence on that point; but so do afferent impulses over the pulmonary fibers of the vagus nerve play an important rôle in regulating the fundamentally automatic discharge from the respiratory center, without being in any way its exciting cause. The work of T. Graham Brown[2] suggests that the same thing is true of the rhythmic movements of locomotion. Brown shows that in a certain stage of ether narcosis in the decerebrate animal, when reflexes can no longer be elicited from the afferent nerves, rhythmic movements of flexion and extension occur in the hind legs; and furthermore, that these movements occur after the afferent nerves from the moving limbs are cut. In other words, these movements which suggest the basis of the movements of locomotion, involving as they do the alternate rhythmic action of antagonistic groups of muscles, are executed by efferent neurones without any stimulation from afferent neurones. They constitute an "endless chain," but not an endless chain of reflexes.

This discovery seems to me to be of sufficient importance to justify dwelling upon it at some length; and in order to obtain a clear picture of the possibilities, we may give briefly Brown's very plausible hypothesis of the nature of the nervous mechanism involved. A movement of this kind consists fundamentally in the alternate contraction of antagonistic groups of muscles. We may denominate the nerve cells innervating each antagonistic group as a half-center, the two together making the entire nerve center for the given movement. Brown supposes that each half-center sends inhibiting collaterals to its antagonist (reciprocal innervation of Sherrington), so that when the flexors, for example, are being excited, the extensor neuro-muscular mechanism is inhibited. He then assumes that the efficiency of this inhibition rapidly diminishes—somewhat as the heart escapes from vagus inhibition—either by the fatigue of the inhibitory mechanism or by the increase of the discharging power of the inhibited cells. The result is that in a short time the inhibited center breaks through its inhibition, excites its muscles to contract and at the same time inhibits the previously active antagonistic half-center.[3] The repetition of these processes leads, of course, to the rhythmic movements referred to.

Brown further raises the very interesting question whether these automatic actions of locomotion do not present a more primitive form of nervous activity than the reflex. He points out the difficulty of imagining the origin of a reflex arc by natural selection, since neither the afferent nor the efferent limb would be of any use to the animal without the other; and it is almost impossible to conceive of both arising at the same time by any assumed process of evolution. It is far more easy to

[2] T. Graham Brown, *Journal of Physiology*, 1914, XLVII., p. 18.

[3] This theory assumes that the cells of both half-centers are automatic and subject to the same environmental conditions (*e. g.*, tension of carbon dioxid) governing their discharge. If both centers were in exactly the same physiological condition and subject to the same environmental conditions, they would discharge simultaneously and alternate rhythmic contractions of antagonists would be impossible. This condition of equal irritability, however, is rarely realized. When it does not obtain, one half-center will discharge first and, as explained, inhibit for the time being the discharge of the other.

suppose that the primitive nervous mechanism is the automatic one seen at work in the movements of narcosis progression. These would serve in the simplest animals the purpose of progression which may reasonably be regarded as among the first functions of coordination a nervous system would be likely to serve. In other words, the nervous mechanism of locomotion, like the nervous mechanism of respiration, is fundamentally an automatic mechanism. Later on afferent neurones are added to it, comparable to those of the pulmonary branches of the vagus. In this connection it is most significant that in general the same conditions so frequently referred to as stimuli of the respiratory center—lack of oxygen, excess of carbon dioxid, etc.—are the very conditions found to favor the movements of narcosis progression.

If, then, to the respiratory center, which has thus far stood in lonely glory as the one fully established example of automatic nervous action, we must add the fundamental centers of locomotion, the thought at once suggests itself that renewed investigation may find the same thing true of other actions which in the past we have too complacently catalogued under the head of reflexes. The field thus opened up is a large one.

AXON REFLEXES

Text-books of physiology usually record two observations, one by Langley, the other by Bayliss, which were not suspected of bearing any relation to each other and both of which have been difficult to fit into the orthodox scheme of nervous action. So far is this true that Langley's axon reflex has been relegated to the inglorious place of a laboratory curiosity which plays no rôle in normal life, while Bayliss's proposed theory of antidromic impulses has been treated with a polite but uncompromising skepticism.

The axon reflex is a reaction made possible by the branching which generally takes place at the end of an axon. Inasmuch as nerve fibers can conduct impulses in both directions, it follows that stimulation of one of the terminal branches will start an impulse traveling up to the point of union of the two branches, and then down the other branch to the end organ. The axons to a frog's sartorius, for example, branch soon after entering the muscle and it often happens that one branch will go to one side of the muscle, while another branch of the same neurone will pass to the opposite side. If, now, the lower third of the muscle be divided longitudinally, it is found that a stimulus applied to one half so as to excite its nerve fibers will cause contraction of the opposite half of the muscle. The same thing is rendered possible whenever a preganglionic efferent neurone passes through several sympathetic ganglia, giving off collaterals to postganglionic neurones in successive ganglia; in this case stimulation of the terminal branch of the preganglionic neurone will start an impulse centripetally and excite, through the collaterals, the cells with which these collaterals are connected. It is also well known that one must be on his guard against axon reflexes in testing the regeneration of nerve fibers, for it often happens that in the process of regeneration an axon of the central stump may branch before entering the peripheral stump; if these two axon branches find their way into different branches of the peripheral nerve trunk, stimulation of one of these branches may give an apparent reflex, which, however, is only an axon reflex.

These and other examples that may be cited are, however, only laboratory curiosities. Where the two branches of the axon end in a muscle or a gland neither branch can be stimulated at its ending except by

artificial means. If, on the other hand, the same axon should send one branch to a sense organ and another to a muscle, or gland, or blood vessel, we would have the possibility of an axon reflex as a normal event. Recent work suggests that this possibility may be realized.

In 1901 Bayliss[4] found that stimulation of the posterior roots of the sacral nerves between the ganglion and the cord produces dilation of the blood-vessels of the hind limb. The natural explanation of the result, that certain vaso-dilator neurones may send their axons out by the unusual path of the posterior instead of the anterior nerve roots, was disproved by the fact that if the posterior roots are cut near the cord and degeneration allowed to occur, stimulation of the peripheral stump of the cut root still produces the dilation. In other words, the ganglion of the posterior root is the trophic center for some of the essential fibers concerned. From consideration of the known histological possibilities Bayliss concluded that the fibers producing the dilation are the ordinary afferent fibers from the pear-shaped cells of the ganglion, the distal axon being supposed to branch at its ending, one branch going to the sense organ, and the other to the blood-vessel. He furthermore supposed that in addition to serving as a trophic center for the afferent fiber, these cells may be reflexly stimulated by other afferent fibers and thus discharge "antidromic" impulses to the periphery; such impulses passing over the branch to the blood-vessel produces the dilation, while the impulse over the branch to the sense organ would be without effect ("law of irreciprocal conduction"). Physiologists have, however, looked askance on this conception of antidromic impulses, even as a working hypothesis.

[4] Bayliss, *Journal of Physiology*, 1901, XXVI., 173; *ib.*, 1902, XXVIII., 276.

The very important experiments of Bruce,[5] however, put the matter in a new light. It is well known that when an irritant is applied to the skin, a dilation of the arterioles (active congestion) ushers in the inflammatory reaction. Bruce shows that this will not occur if the area to which the irritant is applied is first rendered anesthetic with cocaine. It will occur immediately after section of the anterior roots or of the posterior roots, either centrally or distally of the ganglion; hence it is not a reflex through the cord or the ganglion. It will not occur, however, after the completion of the peripheral degeneration consequent upon section of the posterior root distally to the ganglion. In other words, it would seem to depend solely upon the integrity of the distal limb of the neurones of the posterior roots, and to be independent of any nerve cell whatever. This would seem almost to force[6] the conclusion that we are dealing with an axon reflex. The posterior root fiber branches, as Bayliss supposed, at its ending, one branch going to the sense organ while the other serves as a dilator of the arterioles. The same fiber, probably one of pain, which carries the afferent impulse giving rise to the sensation of irritation produces also the active congestion of the region through its vascular collaterals.

If these observations prove well founded, the axon reflex becomes a reality in the normal functioning of the organism, instead of a laboratory curiosity. Moreover, the facts discovered by Bayliss receive their

[5] A. Ninian Bruce, *Quarterly Journal of Exp. Physiology*, 1913, VI., p. 339.

[6] The writer can imagine only one other possibility; namely, collaterals given off from afferent fibers distally to the ganglion may enter the sympathetic ganglia, which would thus serve as reflex centers. No such collateral communications in nerve trunks have, however, been described. Indeed it is the usual teaching that nerve fibers branch only in the ganglion or at their endings.

ready explanation without the help of the hypothesis of antidromic nerve impulses. Furthermore, if the axon reflex is the foundation of this inflammatory reaction, we may well investigate other reactions which in the past have been classed as reflexes, but upon inadequate evidence. So long as the reflex was supposed to be the only means by which stimulation of a peripheral sense organ can evoke a non-volitional reaction in another organ, all such reactions have been classed as reflexes, and this quite frequently without experimental proof.

CONDITIONED AND UNCONDITIONED REFLEXES

The main purpose of this paper is to emphasize neglected aspects of nervous action. Hence our treatment of reflexes, properly so-called, will be confined to emphasizing the fact that we probably include in the category of the reflex two entirely different kinds of nervous reaction.

It is an interesting fact that so common a phenomena as reflex action is somewhat loosely defined in our thinking. One will call it an action brought about by the stimulation of efferent neurones by one or more afferent neurones; another will add to this, "without the intervention of the will"; another will add, "without the causal intervention of consciousness"; while still another will add, "without the causal intervention of consciousness or the will." These four definitions are by no means identical, as I hope to show. All of them have in common the fact of stimulation of efferent by afferent neurones, stimulation being supposed to include both excitation and inhibition, and it being assumed that any number of intermediate neurones (first, second and third order, etc.) may be interposed between the afferent and the efferent nerves concerned. All would exclude the will from any causal connection with the reaction, and this leaves as the chief point of difference in the above definitions the question whether we should exclude from the category of reflex action all cases where the nervous processes concerned in consciousness play, or seem to play, a causal rôle in the chain of events; for there are nervous actions which are in no sense volitional; which have an afferent and an efferent side, and hence resemble reflexes; but in which we encounter the nervous actions concerned with consciousness. The example which at once occurs to us is the so-called psychic secretion of saliva and gastric juice. To these we would add the no less striking case, brought to our attention by Cannon, of the stimulation of the secretion of adrenalin as the result of the major emotions of fear and anger. In all these cases there is the absence of conscious intention; indeed, the subject is unaware that the act of secretion is taking place; and yet the conscious process is the starting point of the efferent discharge. Shall we or shall we not call such actions reflex actions?

The answer to this question is, of course, entirely a matter of arbitrary definition. If we exclude the causal interposition of consciousness[7] from the reflex, such reactions are not reflexes; if we do not exclude it, they are. The decision in such an arbitrary matter, moreover, is determined on purely utilitarian grounds. Definitions exist only to insure clear thinking by keeping separate and distinct those things having some fundamental point of difference. Thus many would have us believe that there is no really fundamental difference between reflex and volitional acts; that the efferent discharge in the simplest reflex is accompanied by a momentary flash of something that corresponds to conscious intention;

[7] To avoid a cumbersome expression, the word "consciousness" is frequently used for the "nervous events connected with the state of consciousness."

and that there are all gradations between this and the highest development of the human will. Perhaps they are right; we will not argue the point; but we nevertheless retain our two categories of reflex and volitional actions, because so long as this conception of volition is a pure hypothesis it is unwise to forget that what are subjectively different may be objectively different as well.

The same principle of definition should be applied to the case under discussion. A reflex from which consciousness is entirely absent and one in which consciousness seems subjectively to play a causal rôle may, from the objective standpoint, be one and the same thing; and yet so long as this is only one of two opposing tenable hypotheses, it would seem to be the sensible thing to make a distinction between them.

One of the world's most eminent physiologists does indeed make such a distinction. I refer to Pawlow's differentiation between the unconditioned and the conditioned reflex. I can hardly think that Pawlow's very striking experiments upon which he bases this distinction are unknown to physiologists generally and all will agree that he is a man whose opinion should command attention; yet I find no notice whatever of this matter in the three admirable text-books of physiology which are most widely used to-day in England and America. Because of this and in view of the fact that this address is to a semi-popular audience I shall go into this matter at somewhat greater length than if I were speaking to specialists in neurology.

The distinction between the conditioned and the unconditioned reflex is well illustrated by the excitation of salivary secretion through the nervous system. When the taste endings are excited by food in the mouth, a purely reflex flow of saliva results. The work of Miller[8] seems to establish the existence of definite bulbar centers for this reaction, the gustatory fibers of the lingual and the glossopharyngeal nerves serving as the afferent neurones. On the other hand, the mouth may water "at the very sight of food." Here the afferent stimulus comes through the optic nerve, but it differs from that through the gustatory fibers in the fact that the reaction is secured only in a conscious animal. It is also more capricious in its occurrence; the whole setting of the nervous system must be right to have it occur at all; the subject must be hungry, the food must be appetizing, it is more apt to occur at the accustomed hour for meals. In short, a certain state of consciousness must exist to insure effective connection between the afferent optic neurones and the secretory efferent neurones. In the unconditioned reflex the nervous processes concerned in consciousness are in no way involved; it will take place in a decerebrate animal and may occur under anesthesia; it is a rare thing that the application of the proper stimulus fails to elicit it, although, like any other reflex, it may be inhibited, as in the old rice test. Above all, it is not easily lost by disuse, perhaps never permanently lost except by some actual atrophy of the neurones involved.

In a remarkable series of experiments Pawlow[9] actually developed in animals conditioned reflexes which could by no possibility have formed part of the previous life of the animal or of its ancestors. Every time a dog was fed, a piece of ice was applied to a certain part of its skin. In the

[8] F. R. Miller, *Quar. Jour. Experimental Physiology*, 1913, VI., 57.

[9] Pawlow, I. P., Huxley Lecture for 1906, *British Medical Journal*, 1906, Vol. II., p. 871; *Lancet*, 1906, Vol. 171, p. 911; SCIENCE, 1906, N. S., XXIV., p. 613; see also Pawlow's articles on the same subject, *Ergebnisse der Physiologie*, 1904, III., 1, p. 177; *ib.*, 1911, XI., p. 345.

course of time (ten days or two weeks) the application of the ice to the same cutaneous area would evoke a flow of saliva without the formality of feeding. The application of ice to other parts of the skin was also effectual, apparently because the sensations of cold concerned in the result were not local, but more or less generalized. In another series of experiments a note of a certain pitch always accompanied the taking of food, and this stimulus, too, after sufficient repetition, could evoke the flow of saliva, while a note of distinctly different pitch was ineffectual. The reactions thus acquired were soon lost with disuse, although it is possible that if the "training" had been continued over much longer periods of time the reactions might have become more firmly fixed; it is even conceivable that they may take place in the absence of consciousness; that is to say, without the participation of cerebral centers; but these are questions which, so far as I am aware, experiment has not yet answered. Finally, they are more or less capricious; not infrequently the acquired response to the stimulus does not occur, thus contrasting with the response to gustatory stimulation, which seldom fails.

In what way is this type of reaction acquired? The phenomenon of reenforcement (of the knee jerk, for example) shows that activity of any one part of the nervous system causes the irradiation over the entire brain and cord of some exciting influence which, though itself minimal or even subminimal, yet adds itself to any other stimulus that may enter about the same time. Pawlow's work seems to show, moreover, that, when two nerve centers are habitually active at the same time, there is beaten out a path of conduction between the two, the two become "associated" so that activity of the one is liable to excite activity of the other. When, for example, the knee jerk is reenforced by stimulus of sound, not only does such an irradiation from auditory centers pass to all parts of the nervous system, the sacral motor centers included, but one also irradiates from the sacral centers to all parts of the nervous system, the auditory centers included; and just as when there are two lights in a room the path between these lights is the most intensely illuminated portion of the room, so in the case in question the path between the two centers is most strongly in the excited state. If now this same combination of activity be repeated over and over again, this path becomes more irritable and conductive by use until we arrive at the condition shown in the above experiments of Pawlow where activity of one center can, of itself, excite activity of the other. It would indeed be interesting to know whether, just as clapping the piece of ice on the skin evoked a secretion of saliva, so the dog experienced a sensation of cold every time he ate.

The path of conduction or association thus established is presumably through the gray matter, perhaps with the help of the short neurones of the border zones.[10] Our present knowledge of the anatomy of the nervous system is inadequate to give a satisfactory idea of the mechanism involved in the development of this new path of conduction; but it is inconceivable that the anatomical basis of the physiological connection between the centers in question should be the same as that pictured in the typical reflex arc of the text-books. Apart from the improbability of the development of new neurones, the observed facts of the capriciousness of the reaction and the ease with which, once acquired, it is lost by disuse determine as the logical course its provisional classification in a group of its own.

[10] One thinks of the "neuropile" of some histologists as a possible tissue in which this path is blazed.

To look at the matter from another point of view, the present state of our knowledge would seem to indicate that the typical reflex of our text-books, the unconditioned reflex, is a congenital mechanism. The neurones concerned and the collateral connections of afferent and efferent limbs are born with us. It may require practise to bring the mechanism into perfect working order; but practise does not produce a new neurone nor have we any reason for thinking it can produce collateral connections which were not already laid down by heredity. The nervous element in locomotion is a case in point. The colt walks from the moment of birth; a human baby not until its second year, and then only after arduous trial and effort; but this does not mean that the nervous mechanism is congenital in the one case and acquired in the other; it merely means that the congenital nervous mechanism is in complete working order at birth in the colt, while in man either embryological development is not complete until later or else use is required to make congenital synaptic connections efficient. Despite the immemorial antiquity of the expression "learning to walk," it may well be questioned whether any child really learns to walk; whether the facts observed are not equally well explained on the theory that the child finally walks simply because at last the embryological development of its nervous mechanism of locomotion is complete, as is that of the colt at birth; and that the improvement which apparently results from its efforts is in point of fact merely the record of the progress of ontogenetic development.

With learning to talk the case is entirely different. Here there is no inherited mechanism leading to a uniform result in all individuals of the species. One child learns to speak English, another German, another Russian; and if the English child had been taken after the first few months of its life to Russia and heard nothing but Russian, it would have learned to speak Russian as perfectly as it actually learned to speak English while growing up in its native country. In this case heredity has furnished a nervous system capable of acquiring just such associations as those described in Pawlow's experiments; we are dealing with a process in every way comparable to the conditioned reflex.

Finally, if the distinction between conditioned and unconditioned reflexes upon which Pawlow insists is correct, some old statements which take us back to our very introduction to the study of physiology need revision, or at least more accurate restatement. When we speak of "habit being an acquired reflex" we really mean an acquired conditioned reflex. There is no reason for assuming that the reflex acquired by the repetition of volitional acts is the typical reflex arc; indeed there is every reason for believing the contrary. Paths of conduction become blazed between different lower centers because they are simultaneously excited in the volitional execution of an action, and a mechanism is acquired of whose nature we know next to nothing, but through which the act can be performed more and more easily with less and less conscious effort—or, in physiological language, with less and less participation on the part of the higher centers of the cerebrum. We are not concerned with the psychology of this phenomenon, much less is this the place for speculation as to the physiological mechanism involved. We are simply concerned with its classification as a distinct thing from the ordinary unconditioned reflex.

Perhaps when introducing this discussion of reflexes I laid undue emphasis on the rôle of consciousness in the acquisition of conditioned reflexes. In the examples

cited the nervous events associated with the state of consciousness do indeed play a conspicuous rôle. If, however, the essential thing about this reaction is what we have suggested, namely, that the connection between afferent and efferent fibers is a path blazed through the nervous substance rather than a definite localized conduction through specialized neurones, it would seem that consciousness comes so frequently into play merely because it is through the nervous substance of the cerebrum that such paths can be blazed most readily, and the activity of cerebral centers carries with it as a usual thing a state of consciousness. If this be true there is no reason why conditioned reflex associations may not arise between subcortical as well as between cortical centers; it is only necessary that the centers be simultaneously active, reflexly or otherwise; and possibly some cases of associated action of two bulbar or spinal centers—respiratory and vaso-motor, or respiratory and cardio-inhibitory—may be of this kind rather than distinct collateral connections between the neurones of the two centers. This is, of course, only a surmise, but it is clearly a possibility and certainly there is no evidence whatever to exclude it. We have been too quick to assume that coordinations are always effected by the same mechanism, and that too the kind of mechanism pictured in our typical reflex arc. An unproved assumption; and so long as it is an unproved assumption it is the logical thing to keep in separate categories the two classes of reactions which to-day are almost universally thought of as one and the same.

Theodore Hough
University of Virginia

THE DEDICATION OF THE NEW BUILDING OF THE MELLON INSTITUTE

The new $350,000 building which will form the permanent home of the Mellon Institute of Industrial Research and School of Specific Industries of the University of Pittsburgh, was formally dedicated on February 26. This building, the gift of Messrs. Andrew William and Richard Beatty Mellon, of Pittsburgh, was especially designed for the needs of the institute; it is distinctly modern in every respect, and complete facilities are provided for the investigation of manufacturing problems and for conducting industrial research according to the practical system of cooperation between science and industry, founded by the late director of the institute, Dr. Robert Kennedy Duncan. By this system, an industrialist having a problem requiring solution may become the donor of a fellowship by providing the salary of the researcher selected to carry out the investigation desired, the institute supplying every facility for the work—laboratory space, the necessary apparatus and supplies, library facilities and advice of a staff expert in industrial research, etc.

The new home of the Mellon Institute is a five-story and attic building. The basement contains seven rooms: the main storeroom, the boiler room, the electric furnace room, a heavy apparatus room, a room equipped for low-temperature work, the machine shop and a kitchen. On the first, the main floor, are located the general office, the director's suite, the office of the editorial department, the library, the office and laboratory of the assistant directors, the assembly hall, a special apparatus room and a dark-room laboratory. The second and third floors each contain ten large research laboratories and nine small ones; the fourth floor, which is not finished, will contain an identical number of laboratories as soon as the growth of the institute warrants its completion.[1] At the present time twenty-three fellowships are in operation and forty research chemists are engaged in a study of the variety of industrial problems under investigation at the institute.

While the Mellon Institute possesses an endowment of its own and has its own board of trustees, it is an integral part of the Univer-

[1] For a full description of the new building of the Mellon Institute, see *The Journal of Industrial and Engineering Chemistry* for April, 1915.

sity of Pittsburgh. The dedicatory exercises were accordingly held in conjunction with the annual charter-day exercises of the university.

The chancellor of the university, Dr. Samuel Black McCormick, presided at the dedication ceremonies, which took place at 11:00 A.M. in Soldiers' Memorial Hall. Following the address of the day by Dr. Rossiter Worthington Raymond, the dean of American mining engineers, on "Knowledge and Research," Dr. W. J. Holland, director of Carnegie Museum and formerly chancellor of the university, made the presentation speech in connection with the dedication of the Mellon Institute, on behalf of Andrew W. and Richard B. Mellon, the donors. After a brilliant eulogy of the Messrs. Mellon and a splendid tribute to their generosity, Dr. Holland said in part:

> In a certain sense, Mr. Chancellor, this building is a memorial to Robert Kennedy Duncan. On one side of the entrance is a bronze slab inscribed with the name of Thomas Mellon; on the other side of the entrance is a bronze slab inscribed with the name of Robert Kennedy Duncan. But, Mr. Chancellor, this splendid edifice erected upon the campus of our university is more than a cenotaph. It not merely commemorates the names and careers of those of whom I have spoken, but it is intended to serve as the seat of advanced inquiries along scientific lines, which will tend to the promotion not merely of intellectual culture, but of industrial success, and that not merely in this great "workshop of the world," where it is located, but throughout the land. In creating this institution our dear friends have been actuated by a high and intelligent purpose. Large experience in great industrial enterprises has taught them the importance of chemistry and physics in their application to the industrial arts, and they feel that, wonderful as has been the progress made within the last century, there are untold mysteries in nature which have not yet been revealed, but which, if uncovered, are capable of being used for the welfare of mankind. And so they have created and are to-day placing in the custody of you, gentlemen of the board of trustees, this institution, which is capable of becoming, when wisely and intelligently administered, a mighty implement for the advancement of human welfare.

Dr. George Hubbard Clapp, president of the board of trustees of the university, delivered the speech of acceptance. He expressed appreciation of the gift and understanding of the importance of the work for which the building has been erected.

The final ceremony of the exercises was the conferring of fifteen honorary degrees, as follows:

Doctor of Laws

Edward Williams Morley, honorary president of the Eighth International Congress of Applied Chemistry.

John Ulric Nef, head of the department of chemistry of the University of Chicago.

Arthur Amos Noyes, professor of theoretical chemistry and director of the Research Laboratory of Physical Chemistry, Massachusetts Institute of Technology.

Rossiter Worthington Raymond, secretary emeritus of the American Institute of Mining Engineers.

Ira Remsen, former president and professor emeritus of chemistry, Johns Hopkins University.

Theodore William Richards, professor of chemistry and director of the Gibbs Memorial Laboratory, Harvard University.

Doctor of Science

John Jacob Abel, professor of pharmacology, Johns Hopkins University.

George Hubbard Clapp, president of the Pittsburgh Testing Laboratory and of the Board of Trustees of the University of Pittsburgh.

Elbert Henry Gary, chairman and chief executive officer of the United States Steel Corporation.

John Hays Hammond, consulting mining engineer.

Henry Marion Howe, former professor of metallurgy, Columbia University.

Doctor of Chemical Engineering

William Hultz Walker, professor of chemical engineering, Massachusetts Institute of Technology.

Milton C. Whitaker, professor of industrial and engineering chemistry, Columbia University.

Doctor of Chemistry

Charles Lathrop Parsons, chief mineral chemist, Bureau of Mines.

Edgar Fahs Smith, provost of the University of Pennsylvania.

Immediately after the close of the dedicatory exercises, the trustees, faculty and guests of the university met at a luncheon in the University Club. The remainder of the after-

noon was devoted to an inspection of the new building of the Mellon Institute.

The recipients of honorary degrees were the guests of the university at the annual alumni banquet held at the Schenley Hotel from 6:00 to 8:30 P.M. The speakers at this dinner were Dr. Raymond F. Bacon, director of the Mellon Institute, who responded to "The Mellon Institute"; Dr. Walther Riddle, who gave a historical sketch of the department of chemistry of the university; Hon. Elbert H. Gary, chairman of the United States Steel Corporation; Dr. Theodore William Richards, who spoke on "The Practical Use of Research in Pure Science" and extended Harvard's congratulations to Pittsburgh upon the acquisition of the Mellon Institute; and Chancellor Samuel Black McCormick, who completed the toast list with an eloquent response to "The University," in which he stated that the gift of the Mellon Institute had placed a great responsibility upon the University of Pittsburgh as well as having been a priceless acquisition; that the university was ready to meet the responsibility and, he felt sure, would show the donors and the country at large that it would make the most of the great benefaction.

Judge Gary's address was in part as follows:

> In humankind there is an element which is interested in, if, indeed, it does not actually enjoy reading or listening to, adverse references to the character or conduct of an individual or association of individuals, and, by reason of this fact, agencies for the collection and distribution of unfavorable comments have become more or less popular. A questionable kind of success is often realized by the individual or the publication whose energies are devoted to frequent and furious personal attack against the standing or the action of others. These efforts sometimes take the form of individual work, investigations by committees or commissions created by the legislatures or congresses, or, in exceptional cases, even by judicial branches of government, such as grand juries, with their inquisitorial power. Oftentimes the investigators are not only utterly incompetent, but they are prejudiced and willfully repress many of the pertinent and material facts. They seek to produce for circulation and criticism only information calculated to bring reproach upon the persons involved in the inquiry. No one is exempt from these criticisms.
>
> Circumstances seem to show that we are approaching the time when the investigator will be investigated; when the criticizer will be criticized; when committees and commissions will be brought before other similar bodies for judgment. It would be interesting to the public if it could be informed of the real motives which have prompted some of the official inquiries, and if it could learn of the unfair methods which have been sometimes pursued, and if it should know the amount of governmental funds which have been appropriated for the use of committees and how they have been disbursed; in fact, if some of those participating could be subjected to the same scrutiny which they have exercised.
>
> The general attitude of the great newspapers of to-day is fair and just. They influence and are influenced by the general public. They reflect the general sentiment. This is most important in considering the future welfare of this country.
>
> If the picture which I have drawn is a true one, then the course before us, which leads to prosperity, success and happiness, is plain and we will pursue it. We must conduct affairs in our charge with the expectation that we shall be criticized.

After the banquet, the new building of the Mellon Institute was thrown open for a reception of friends of the institute. The rooms of the main floor were used for the reception, although the entire building was open for inspection. On the evening of February 27, the first Mellon lecture was delivered by Professor John Jacob Abel, of Johns Hopkins University, in the assembly hall of the institute; Dr. Abel's subject was "Experimental and Chemical Studies of the Blood and Their Bearing on Medicine."

W. A. Hamor

The Mellon Institute of Industrial Research

CHARLES EDWIN BESSEY

The Botanical Society of Washington at its one hundred and third regular meeting, March 2, 1915, unanimously passed the following resolutions upon the death of Doctor Charles E. Bessey, dean and professor of botany at the University of Nebraska.

Whereas, In the recent death of Professor Charles E. Bessey, botany has lost one of its ablest investigators and teachers, one of the pioneers in

the introduction of the present laboratory methods of teaching biology, and

WHEREAS, He was widely known and highly respected by his fellow botanists, and beloved by his numerous students in whose welfare and success he took the keenest personal interest, and

WHEREAS, The death of such a man comes as a personal loss to all American botanists; therefore be it

Resolved, That the Botanical Society of Washington express its deep sense of regret and extend to his family its deepest sympathy in their great bereavement; be it further

Resolved, That a copy of these resolutions be spread upon the minutes of this society and sent to the immediate relatives of the deceased and to SCIENCE.

PERLEY SPAULDING,
Corresponding Secretary

SCIENTIFIC NOTES AND NEWS

THE annual general meeting of the American Philosophical Society will be held in the hall of the society at Philadelphia on April 22, 23 and 24, beginning at 2 P.M. on Thursday, April 22.

SIR J. J. THOMSON has been elected president of the Physical Society, London. Prince B. Galitzin has been made an honorary fellow of the society.

THE gold medal of the British Institution of Mining and Metallurgy has been awarded to Dr. Willet G. Miller, geologist of Ontario.

AT the meeting of the Royal Geographical Society on February 22, Mr. Page, the American ambassador, presented to Dr. Scott Keltie, secretary of the Royal Geographical Society, the Cullum gold medal, awarded to him by the American Geographical Society.

MR. JOHN S. LONGWELL, C.E. (Cornell, '10), of the Reclamation Service, has been awarded the prize given annually by the American Society of Civil Engineers for the best paper by a junior member of the society.

THE Samuel D. Gross prize of the Philadelphia Academy of Medicine for the year 1915 has been awarded to Dr. John Lawrence Yates, Milwaukee, for his essay entitled "Surgery in the Treatment of Hodgkin's Disease." The amount of this prize is $1,500.

MR. W. H. HOYT, C.E. '90, College of Engineering, University of Minnesota, assistant chief engineer of the D. M. N. Railroad, has been elected president of the Minnesota State Surveyors and Engineers Society.

THE Atlanta Neurological Society was organized February 11, with the following officers: *president,* Dr. E. Bates Block; *vice-president,* Dr. Hansell Crenshaw; *secretary,* Dr. Lewis M. Gaines. The society will hold its meetings on the second Thursday of each month.

THE *Journal* of the American Medical Association states that Dr. William J. Mayo, of Rochester, Minn., was the guest of honor at the Detroit Academy of Medicine on February 23. On February 22, Dr. Mayo was the principal speaker at foundation-day exercises of the department of medicine and surgery at the University of Michigan, Ann Arbor, and on February 24 the Kalamazoo Academy of Medicine gave a luncheon in honor of Dr. Mayo, after which he gave an address on "Some General Considerations which influence the Advisability of Surgical Treatment." Dr. Charles H. Mayo was the guest of honor at the twentieth annual banquet of the Kansas City Academy of Medicine, February 27. Dr. Jefferson D. Griffith presided and Dr. Mayo delivered an address on "Why Gastro-Enterostomy Fails to Cure."

THE Harvard University unit for service at the American Ambulance Hospital in Paris left on March 17, to serve until June 30. Members of the unit are: H. Cushing, '95, M.D., A.M., Moseley professor of surgery, as head surgeon; R. B. Greenough, '92, assistant professor of surgery, surgeon and executive officer; R. P. Strong, professor of tropical medicine, bacteriologist; R. B. Osgood, M.D., '99, instructor in orthopedics, orthopedic surgeon; B. Vincent, '98, assistant in surgery, assistant surgeon; W. M. Boothby, '02, lecturer in anesthesia, anesthetist; F. A. Coller, '12, M.D.; E. C. Cutler, '13, M.D.; P. D. Wilson, and M. N. Peterson, '14, M.D., resident surgeons; L. G. Barton, Jr., '12, M.D., surgical assistant; O. F. Rogers, Jr., '12, M.D., medical

assistant; G. Benet, '13, M.D., laboratory assistant; and Miss Edith I. Cox, Miss Geraldine K. Martin, Miss Helen Park and Miss Marion Wilson, operating nurses.

Dr. R. A. Reeves, professor of opthalmology in the University of Toronto, president of the British Medical Association for the meeting held in Toronto in 1906, has become professor emeritus.

Dr. B. H. A. Groth has resigned his position as plant physiologist in the department of botany of the New Jersey College experiment Station, to become director of the experiment station under the government of the Republic of Panama. After April 15 he will be in Panama City.

Dr. Karl Van Norden, formerly in the research department of John Hopkins Hospital, who has been an officer in the German army since the beginning of the war, was seriously wounded at the battle of Lodz but is now about to return to the front.

Dr. E. O. Hovey, of the American Museum of Natural History, has gone to the West Indies to continue the studies on the volcanoes of the Lesser Antilles, which he began some years ago when the great eruptions on the islands of Martinique and St. Vincent occurred. He will devote his time particularly to the Grande Soufrière of Guadeloupe, Mount Pelé of Martinique, the Soufrière of St. Vincent and the boiling lake of Dominica, collecting gases from the fumeroles and making temperature observations, and taking note of the changes which have occurred since his visit in 1908. The expedition is undertaken through the aid given to the museum by the Angelo Heilprin Exploration Fund established by Mr. and Mrs. Paul J. Sachs.

The departments of geology of Harvard University and Massachusetts Institute of Technology announce that Dr. Ralph Arnold will give a series of ten lectures on the "Geology of Petroleum." The first five lectures will be given from 4:30 to 5:30 P.M. on April 5, 6, 7, 8 and 9 in the geological department at Harvard. The last five lectures will be given from 4:30 to 5:30 P.M. on April 12, 13, 14, 15 and 16 in the geological department of the institute.

F. C. Langenberg, of Harvard University, and R. G. Webber, of Ohio University, Athens, Ohio, at the recent New York meeting of the American Institute of Mining Engineers read a paper on the Structure and Hysteresis Loss in Medium-Carbon Steel. It was illustrated by microphotographs of the physical structure, and curves of the hysteresis loss in a series of steels heat-treated to different temperatures.

Professor C. F. Shoop, of the Experimental Engineering Department of the College of Engineering of the University of Minnesota, recently read a paper before the Minnesota Society of Engineers and Surveyors in annual convention in St. Paul. The title of the paper was "The Abrasion Value of Various Concrete Aggregates in Concrete Roads."

A memorial tablet has been placed in the house at Cosenza, Italy, where the eminent alienist, B. Miraglia, was born, and a similar tablet is to be placed in the insane asylum at Aversa, the scene of his work, and a street in Aversa is to be named after him.

Miss Davy, niece of Sir Humphry Davy, has presented to the Royal Institution, London, a bust of the great chemist executed by Samuel Joseph in 1822.

Frank Asbury Sherman, professor of mathematics at Dartmouth College from 1871 until his retirement as professor emeritus in 1911, died on February 25 in his seventy-fourth year.

Sir George Turner, distinguished for his work on the rinderpest and on leprosy, died on March 12 at the age of sixty-four years from leprosy, contracted during research work to discover a cure for the disease.

Dr. E. von Esmarch, formerly director of the Kaiser Wilhelm Institute at Dresden, died on February 5, at the age of fifty-nine years.

Dr. Julius Arnold, professor of pathological anatomy at the University of Heidelberg, died on February 6, in his eightieth year.

The Washington Academy of Sciences held a joint meeting with the Biological Society

on March 11, in the Auditorium of the new National Museum, when there was a lecture by Mr. Wilfred H. Osgood, of the Field Museum of Natural History, who was engaged on a special investigation of the fur-seal question for the Department of Commerce during the summer of 1914. His subject was "Fur Seals and Other Animals on the Pribiloff Islands."

DIRECTOR JOHN F. HAYFORD, of the college of engineering, Northwestern University, addressed a group of graduate students and professors of the University of Wisconsin on the subject of "Isostasy" on March 1. That evening he addressed the Science Club of the same institution on the decision in regard to the Panama-Costa-Rico Boundary Dispute. Director Hayford was chairman of the committee appointed by the chief justice of the United States, which made a personal investigation and survey.

DR. LAFAYETTE B. MENDEL, professor of physiological chemistry in Yale University, addressed the Johns Hopkins Hospital Medical Society at Baltimore, March 1, on "Nutrition and Growth."

THE tenth lecture before the Harvey Society was given on March 13, at the New York Academy of Medicine, by Professor Elliott P. Joslin, of Harvard University, on "Carbohydrate Utilization in Diabetes, based upon Studies of the Respiration, Urine and Blood."

DR. CHARLES S. BERKEY, associate professor of geology in Columbia University, will give the last of the Jessup lectures on "Origin and Meaning of Some Fundamental Earth Structures" at the American Museum of Natural History on March 26. The subject of the lecture is "The Relation of Structural Geology to Practical Undertakings."

PROFESSOR EDWARD H. WILLIAMS, JR., of Woodstock, Vt., for many years head of the department of mining and geology at Lehigh and now a lecturer of the university, gave two lectures in February before the students. His subjects were "The Geology of the Lehigh Valley" and "The Formation of the Allegheny River."

THE American Association of Pathologists and Bacteriologists, of which Dr. Leo Loeb, St. Louis, is president, will meet in St. Louis, on April 2 and 3. The meetings will be held in the pathological department of Washington University Medical School and in the library of the St. Louis University. Preceding these meetings on April 1 will be held the annual meeting of the American Association for Cancer Research and the annual meeting of the International Association of Medical Museums. These meetings will be held in the laboratories of the Washington University Medical School.

THE seventh semi-annual meeting of the American Institute of Chemical Engineers will be held in San Francisco, Calif., from August 25 to 28. An itinerary is being arranged so that the natural scenery of the west may be seen and also some of the more important mining operations as well as the typical chemical industries of California.

THE senate of the Kaiser Wilhelm Society for the Advancement of Science at a session held on January 23, determined to break ground for the projected Kaiser Wilhelm Institute for Physiology and for the Study of the Brain. The Kaiser Wilhelm Institute for Biology is soon to be opened at Dahlem.

UNIVERSITY AND EDUCATIONAL NEWS

AT the convocation at the University of Chicago, on March 16, Julius Rosenwald Hall, devoted to the work of the departments of geology and geography, was dedicated. The building, a gift from Mr. Julius Rosenwald, a trustee of the university, has cost approximately $260,000.

THE Arnold Biological Laboratory, ground for which was broken at Brown University last summer, is practically completed and will be put into use for regular class work with the reopening of college after the spring recess. The building, which is three stories in height, 117 feet long and 52 feet wide, will cost when finished $80,000, and $30,000 more will be expended upon the equipment. The cost of the building will be covered by a bequest made to the university for the purpose by the late Dr.

Oliver H. Arnold, while the funds for equipment have been subscribed.

Professor Thomas S. Fiske has been designated as administrative head of the Columbia University department of mathematics for two years beginning July 1, in the place of Professor Cassius J. Keyser, who retires at his own request.

Mr. Morris M. Wells, of the University of Illinois, has been appointed instructor in the department of zoology in the University of Chicago.

The Benjamin Peirce instructorships in mathematics at Harvard University, the terms of whose establishment were recently announced in Science, have now been filled for the year 1915–16 by the appointment of Dr. Edward Kircher and Dr. George A. Pfeiffer.

DISCUSSION AND CORRESPONDENCE

THE FUNDAMENTAL EQUATION OF MECHANICS

To the Editor of Science: Professor Huntington's letter in Science of February 5 is an important contribution to the subject of the teaching of elementary dynamics, but the fact that he and Professor Hoskins are not in agreement on "the question whether $F=ma$ or $F/F'=A/A'$ is the better form in which to introduce the fundamental equation of mechanics" shows that something remains to be said on the subject. In my opinion neither of these equations ought to be considered as fundamental, for both are derived from more elementary equations.

Professor Huntington objects to $F=ma$ for certain reasons. He might have made other objections to it: for example, the equation is not true in the ordinary English system (foot-pound-second) until it is hybridized by valuing either F or m in some other unit than pounds (poundal or gee-pound) or a in "gravitals" (instead of feet) per second per second (1 gravital = 32.174 feet),[1] or else the letter m is explained as not being quantity of matter in pounds, but only the quotient or ratio W/g. Neither is it true in the metric kilogram-meter-second system. (I do not think the metric people have yet tried to introduce a "kilogrammal" or a "gee-kilogram.") It is of course true in the dyne-centimeter-gramme-second system, but this system is only used in higher physical theory, and it should not be inflicted on young students. The equation $F=ma$ is, however, a handy equation to work with when it is understood that m is merely a conventional symbol for W/g.

The equation $F/F'=A/A'$ may be useful for some purposes, but I agree with Professor Hoskins in not accepting it as fundamental or as the best equation to be used as an introduction of the subject. Each of the equations being open to objection, I wish that both Professor Hoskins and Professor Huntington would consider the following treatment of the subject, and let me know what objections there are to it.

Quoting Professor Huntington's words: "The first serious problem which confronts the teacher of dynamics is the problem of making the student understand the effect which a force produces when it acts on a material particle" (I would substitute the word "body" for material particle).

Let us start with the student just out of the grammar school, who has never studied physics, but who understands the simplest forms of algebraic equations, and how to make $a=F/m$ out of $F=ma$. He already knows the ordinary meaning of the words time, space, force, matter (or stuff, solid, liquid or gas). He may be told that the word "body" means a piece or chunk of stuff, and that velocity is just another name for speed. He knows that force may be measured by a spring balance, and that the quantity of matter in a body may be determined by weighing it on grocer's even-balance scale or on a platform

[1] The writer invented the "gravital" and also the "timal" (=1/32.2 of a second) over 20 years ago as antidotes to the "poundal," merely to serve as "horrible examples" of what might be done in the way of introducing still further confusion into our systems of units. He also invented the "massal" = 32.2 pounds, but that has got into some text-books disguised under the names of "gee-pound," slug, and "engineers unit of mass." The latter term is especially objectionable, for it has never been used by engineers.

scale; provided it is weighed at any place other than the imaginary "point of zero gravity."

The fundamental problem to be considered by the student is: Given a constant force F lbs. acting for T seconds on a quantity of matter W lbs., at rest at the beginning of the time, but free to move, what are the results, assuming that there is no frictional resistance?

The first result, which is already known by the boy, is motion, at a gradually increasing velocity. What the relation is between the elapsed time and the velocity may be determined by experiment. He may take a moving picture, with 50 films per second, of a body falling alongside of a rod marked with feet and inches. He may tow a boat having a load of 1,000 lbs. with a force of say 1 lb., exerted through a string and measured by a spring balance, alongside of a tow path on which a tape line is stretched; or there may be an Atwood machine in the high school on which experiments may be made. By these experiments he will learn the fundamental facts of dynamics and establish the fundamental equation. The facts are that the velocity varies directly as the time and as the force, and inversely as the quantity of matter, and the equation is $V \propto FT/W$ or $V = KFT/W$, K being a constant whose value is approximately 32, provided V is in feet per second, F and W in pounds and T in seconds.

The accurate determination of K requires the most refined experiments, involving precise measurements of both F and W, and of S, the distance traversed during the time T, from which V is determined, and precautions to eliminate resistance due to friction of air or water or of the machine used in the experiments. When these refined experiments have been made it has been found that the value of K is 32.1740, and this figure is twice the number of feet that the body would fall *in vacuo* in one second at or near latitude 45° at the sea level. It is commonly represented by g, or by g_o, to distinguish it from other values of g that may be obtained by experiments on falling bodies (or on pendulums) at other latitudes and elevations. The fundamental equation then is $V = FTg/W$ (1)

The velocity V is a derived quantity, derived from measurements of space (or distance) and time. If a body is moving at a uniform speed, such as the minute hand of a watch, V is a constant, and the distance varies directly as the time, and is the product of the velocity and the time, $S = VT$. But if the velocity varies directly as the time (uniformly accelerated motion), as in the case of the problem we are considering, then the distance is the product of the mean velocity and the time. Since in our problem the body starts from rest when the velocity is 0, and the velocity is V at the end of the time T, the mean velocity is $\frac{1}{2}V$ and the distance is $\frac{1}{2}VT$, whence $V = 2S/T$ and $T = 2S/V$.

The velocity V in feet per second, at the end of the time T is numerically equal to the number of feet the body would travel in one second after the expiration of the time T if the force had then ceased to act and the body continued to move at a uniform velocity.

The fundamental equation might be written $2S/T = FTg/W$, which is equivalent to $S = FT^2g/2W$, but as this is somewhat more cumbersome than the simpler-looking equation $V = FTg/W$, this latter equation is more convenient as the fundamental equation. It expresses the facts that the velocity varies directly as F and T and inversely as W, and that the velocity equals the product of F, T and g divided by W. Let us further consider the two equations $V = FTg/W$ (1) and $S = FTg/2W$ (2).

We have dealt with four elementary quantities F, T, S, W, one derived quantity V, and one constant figure 32.1740. It is understood that F is measured in standard pounds of force, the standard pound of force being the force that gravity exerts on a pound of matter at the standard location where $g = 32.1740$.

Each equation contains four variables V, F, T, W, or S, F, T, W, and in either equation if values be given to any three out of the four the fourth may be found. By ordinary algebraic transposition, or by giving new symbols to the product or quotient of two of the variables, many different equations may be derived from them, some of which are more curious than

useful. It is well not to give the student too many of them or he will become confused.

Here are some conclusions that may be derived from the equations, (1) and (2).

From (1), let $F=W$, the case of a body falling at latitude 45° at the sea level; then $V=gT$. If T also $=1$, then $V=g$, that is the velocity at the end of 1 second is g.

In the equation $V=gT$ substitute for T its value $2S/V$ and we have $V=2gS/V$, whence $V^2=2gS$. In the case of falling bodies, the height of fall H is usually substituted for S, and we obtain $V=\sqrt{2gH}$ (3).

Equation (2) with $F=W$ gives $V=\frac{1}{2}gT^2$.

From (1), by transposition we may obtain $FT=W\times V/g$ (4). The product FT is sometimes called impulse, and to the expression $W\times V/g$ is given the term momentum. It is usually written W/gV, but there is no reason why, except that it is customary, and it has been found convenient to use the letter M instead of W/g, so that the equation becomes

$$FT = MV \qquad (5)$$

Impulse = Momentum

In (4) we may substitute for T its value in terms of S and V above given, viz., $T=2S/V$ and obtain $F2S/V=MV$; whence $FS=\frac{1}{2}MV^2$ (6). The product FS is called work, and the expression $\frac{1}{2}MV^2$ kinetic energy, whence work expended = kinetic energy.

Acceleration.—The quotient V/T is called the acceleration. It may be defined at the rate of increase of velocity, the word rate, unless otherwise stated, always meaning the rate with respect to time, or "time-rate." In the problem under consideration, the action of a force in a body free to move, with no retardation by friction, the acceleration is a constant, $V/T=A$. The quantity g is commonly called the acceleration due to gravity, but it also may be considered either as an abstract figure, the constant g in equation (1), or as the velocity acquired at the end of 1 second by a falling body, or as the distance a body would travel in 1 second at that same velocity if the force ceased to act and the velocity remained constant.

Equation (6) then may be written

$$F=MA \qquad (7)$$

Force = M times the acceleration.

If a given particle [body] is acted on at two different times by two forces F and F', and if A and A' are the corresponding accelerations, then

$$\frac{F=MA}{F'=MA'} \text{ whence } F/F'=A/A'. \qquad (8)$$

Equation (7) is called the fundamental equation by Professor Hoskins, while equation (8) is called fundamental by Professor Huntington, but it is shown above that they are derived from the more fundamental equation $V=FTg/W$.

Summary.—Take equation (1), $V=FTg/W$ (1). Substitute $2S/T$ for V, $S=FT^2g/2W$ (2).

Take $F=W$, then $S=\frac{1}{2}gT^2$,

and $V=\sqrt{2gH}$ (3)

From (1) by transposition $FT=WV/g$ (4)

Substitute M for W/g, $FT=MV$ (5).

In (5) substitute $2S/V$ for T,

$$FS=\tfrac{1}{2}MV^2 \qquad (6)$$

In (5) substitute A for V/T, $F=MA$ (7)

Apply (7) to the case of two forces acting at different times on the same body

$$F/F'=A/A' \qquad (8)$$

In this treatment the ambiguous words "weight" and "mass" have purposely been omitted.

If there is any easier way of "making the student understand the effect which a force produces when it acts on a material particle" than to have him study the above discussion and solve examples by its aid, it is very important that it should be found and incorporated in the text-books.

Wm. Kent

A COURSE IN AGRICULTURE FOR NON-TECHNICAL COLLEGES

That there is an interest in agriculture as a subject of study in colleges or higher institutions in addition to that met by the state agricultural colleges, is manifested by the introduction a few years ago into the curriculum, in certain institutions (*e. g.*, Syracuse and Miami Universities) of several subjects associated with the work of the land-grant colleges. Further evidence is shown in the

preparation by one of the professors of a text on agricultural education which is regarded as well toward the head of the list upon that subject. But there seems to be still a field for educational work in agriculture, apparently not touched by any of the current courses, by which the subject matter of botany, zoology, geology and meteorology can be correlated with history through the common ground of agriculture.

The recent article upon agricultural botany, by Dr. Copeland, in SCIENCE, September 18, has suggested some details of such possible correlation in addition to a general plan already in mind. The scope of the course in mind is just the reverse of the work as ordinarily catalogued as a "Course in Agriculture" in the state colleges. Such courses take the general subject, agriculture, and divide it into its component parts, assigning portions to agronomy, to horticulture, to animal husbandry, soils, farm management and the other familiar departments. The other plan would take the work in the botanical laboratory and would show where it is of common application in the regular work of the farm; and in zoology, why the domestic animals are so useful to man through their anatomy and physiology, in place of merely noting their places as mammals in taxonomic scheme. It would show that the development of the technique of agriculture has been the companion, if not the guide, to advancing civilization through ethnology and anthropology to modern history, commercial and industrial.

This is an ambitious aim and would require much careful selection of material, before it could be regarded as definitely outlined. The final form would be an adjustment of the ideas of several rather than the dictum of an individual, as has been the case with college-entrance requirements in the sciences, although no official sanction, outside the several institutions which might offer the course, would be called for. As here outlined, the principal work of the course would be cared for by the regular staff of instructors in botany, zoology, geology, etc., the specialists in agronomy, live-stock or soils being left with their respective subjects in the technical school. Under such regular teachers, however, those details in their course which relate to agriculture in any manner are to be brought out and made the peg upon which to hang the several facts of structure, behavior or adaptation observed.

The field as a whole may be divided into four sections, as follows:

1. *Soil.*—The basis of agricultural activity. Origin of soils; types of soil; properties of different soils; soil biology; soil management.

2. *Plants.*—The factory of agricultural products. Seeds; growth; nutrition; reproduction; weeds and diseases; phytogeography; agricultural ecology.

3. *Animals.*—The product of agricultural factory. Nutrition; anatomy; physiology; breeds; uses; predacious and beneficial species.

4. *Man.*—The controlling factor in agriculture. Races, civilization, colonization; commerce; rural and urban; raw materials and manufactures.

In attempting to assign to these topics their places in the four-year course, it must be remembered that it is not practical agriculture, but fundamental agriculture, that is in mind; it is not an attempt to make farmers, but to show how the farmer gets the results he does from certain methods of procedure, and why he is using those methods instead of some others, in a historical and economic, rather than technical and special study. Thus under the subject of soils, the danger of severe washing of fall-plowed fields in the south would be contrasted with the beneficial effects of the frost work on similarly treated fields in the colder states. Through the aid of the departments or instructors in bacteriology and mycology, relation between soil bacteria, root-infesting fungi and other organisms could be shown as scientific reasons behind the observed benefits of crop rotation, thus connecting the work on soils with that on crop plants. In the consideration of plants, the fact that upon green plants all animal life depends is the keynote, with details added discussing the parts of such plants utilized in particular cases, thus connecting directly with the study of those animals which make direct use of plant tissues for food. Under animals, the adaptation of the teeth to hard-stemmed forage

plants and the ease with which such crops are raised, should be brought out as important details in the usefulness of horses and cattle, as well as their anatomical adaptation to the work of pulling or carrying loads, and their physiological adaptations for meat and milk production.

The treatment of man as outlined would involve as much of the advanced sciences of anthropology and ethnology as one had time for; would naturally involve ancient history, in connection with grain commerce of Rome and her colonies; would take up the development of agricultural communities through the feudal system to the modern village of tenant farmers, and the rise of the freeholders, especially in the new settlements. The relation of established feast days (*e. g.*, Feast of First Fruits) of the ancient tribes, to events of the agricultural year would introduce the religious side of man, and the importance of conserving the produce of his labor, would serve to connect the ideas of property, ownership, wealth, capital and law.

The simpler relations would naturally be assigned to the earlier years of the course. Thus the subjects relating to plant life, in their fundamental details could be given in the sophomore year, supplementing the freshman work in botany; some work would likely be well retained to a later period. Soil work should follow the first year's work in chemistry and in physics, as the general properties of soils are in accordance with the principles learned in those subjects. The study of animals in relation to agriculture would be a good junior subject, as the additional year of work would make it the easier for the student to follow the course, and to grasp the essential points of structure, behavior, conditions of existence among wild and domestic animals, and similar details after he has had the less complex relationships among plants brought out in the sophomore work.

The study of the relation of man to agriculture, as suggested, should come in the senior year, in order that the work in history, economics, engineering and science may be available for use to aid in the development of the course by each man in the class bringing to it as broad a basis of work as possible. The topics introduced at this stage might easily serve as the basis of further study by the few specially interested along the lines of colonial, economic or industrial development. Frequent assignments of readings would be necessary, as the material is scattered and must be brought together under the new viewpoint.

Some suggestions have been found in several text-books on agriculture, agricultural education, farm management and similar topics, more or less along the lines here suggested, but in most cases, the discussion was from the standpoint of technical agriculture, as would be expected. Particular chapters could, however, be selected from a number of such books, to be used as collateral reading by either of the four college classes, suitably supplemented by lectures presenting the desired viewpoint, and developing the central theme. This may be briefly stated as follows: Agriculture as the oldest industrial occupation of man is the basis of all his later achievements, and supports him in his highest attainments. The course might be designated as one in "The development and scope of agriculture" and could be a lecture course supplemented by specified laboratory and class work in the several departments involved. The course might also be developed as a series of short courses, something on the plan of the "summer school" work, correlated by a carefully prepared syllabus or outline, each teacher selecting those phases of the work most closely related to agriculture in its broadest sense, and emphasizing the relation of his subject to the general topic.

Frederick H. Blodgett

SCIENTIFIC BOOKS

Psychology: General and Applied. By Hugo Münsterberg. New York and London, Appleton, 1914. Pp. xiv + 487.

Professor Münsterberg's latest work breaks away from the traditional presentation of psychology in many respects. The most novel features are the author's treatment of mental data from the teleological standpoint and the

emphasis which he lays on applied psychology. Each of these aspects of the subject is developed at considerable length.

The main body of the work is devoted to scientific psychology; but even here the treatment is out of the ordinary. We miss the usual detailed description of the nervous system and end organs. The author expresses his conviction in the preface that details from accessory sciences such as anatomy do not belong in an outline work on psychology. On the other hand, he believes that psychology should embrace social as well as individual phenomena, and accordingly several chapters are devoted to an examination of mental processes in the social group. Professor Münsterberg does not venture into the field of animal psychology, but he gives considerable prominence to "behavior" in the human sphere. In this connection he points out that tools are human extensions of the motor end-organs, while language is a highly specialized motor function, comparable however with other forms of motor activity.

After defining the scope of psychology in two opening chapters, the author proceeds to the scientific description and explanation of mental events. This aspect of the subject he terms *causal* psychology, to distinguish it from the purposive treatment which follows. More than half of the volume is devoted to the causal presentation, which for most writers constitutes the whole of scientific psychology. This part of the work is exceptionally clear and readable. One is reminded of the author's late colleague, William James, whose interesting style and picturesque illustrations add much to the value of his classic text.

It is to be regretted that Professor Münsterberg has not imitated his predecessor's fullness of treatment as well. In endeavoring to compress his material within too narrow limits he is compelled to curtail the discussion of certain topics unduly. For example, one would desire a more exhaustive examination of imagery, discrimination, abstraction and reasoning than the volume supplies. In this part of the work the author insists on a thoroughgoing scientific procedure. His psychological analysis rests on a rigid psychophysical basis and he aims at a complete mechanistic explanation of mental phenomena through the physiological processes which accompany them.

Professor Münsterberg classifies the elementary psychophysical processes under four heads: stimulation, association, reaction and inhibition; the complex processes include perception, ideas, activity, inner states and personality. A striking feature here is the grouping of actions, attention and thought processes together under the head of *activity*. Inner states are divided into simple feelings of pleasure and displeasure, emotions, and esthetic and intellectual attitudes.

The transition from individual to group processes is made through the study of race, sex, age and individual differences. It may be questioned whether such variations do not belong more properly to comparative than to social psychology; but as the author points out, the differences among individuals facilitate their grouping into social unity. The social grouping itself depends upon three elementary processes: union, submission and aggression. These factors work together and result in the complex social processes of organization and achievement.

The second part of the work is devoted to purposive psychology. Here the object is not to describe the inner life, but to understand its meaning. By a curious *volte-face* the author discards the scientific explanation of mental phenomena which he has hitherto insisted upon rigorously, and considers only their teleological bearings. Psychology regarded from this standpoint is "entirely removed from the world of describable objects and understood as an account of those functions in the personality which point beyond themselves and are felt as deeds of the subject" (46). In connection with this change to the subjective standpoint Professor Münsterberg renames the facts themselves. Instead of psychical elements we have experiences; instead of perception we have immediate reality; ideas become meaning, activity becomes the will.

It is somewhat difficult to grasp the significance of this transformation. Granting that a plexus of ideational elements may be called

meaning, and that a certain plexus of activities constitutes *will,* the scientist may still question the propriety of abandoning the associational basis of meaning or ignoring the causal sequence of volitional acts, as Professor Münsterberg appears to do.

In other sciences the speculative hypotheses which have stood the test of criticism have been attempts to amplify or reconstruct the principles discovered by the science itself, rather than to deny its fundamental generalizations. Professor Münsterberg's reconstruction of psychology, on the contrary, starts out by repudiating the generalizations based on observed temporal sequences, and assuming that the acts of our inner life are not contained in time (301), that "our mental life is free" (296).

It would appear that the author makes altogether too crucial a distinction between *cause* and *purpose.* His interpretation of both terms is open to challenge. The analysis of the *purpose* concept has never been fully carried out, but at least we know that "prevision" and "activity toward an end" admit of biological interpretation in harmony with mechanistic principles. As for causality, the author's use of the concept is not in harmony with Hume's classic analysis, which demonstrated that "necessary" connection is not an essential feature of the causal sequence.

Science to-day generally accepts Hume's conclusions. The chemist and physicist regard the laws of their sciences as merely generalized statements of observed facts. They distinctly refuse to commit themselves as to whether causal sequences *must* be as they actually are. Since Darwin's time most biologists have interpreted the evolution of species and the stages of individual development in the same way. Scientific explanation at the present day does not seek to impose anthropomorphic compulsions upon nature. Nature has been found to be self-consistent in the past; the scientist assumes that the same self-consistency will be observed in the future. The generalized notion of uniformity and self-consistency is all that is implied in the scientific conception of *law.*

Professor Münsterberg interprets the term "law" as involving a "necessary connection" between phenomena. For example, if we have met a man and heard his name, "the law of association makes it necessary that if we meet the man again his name comes to our mind" (22). The author states specifically that "the scientist has a right to claim that all his laws are meant as expressions of causal necessity" (31). Yet this necessary connection is just what most physical scientists plainly disavow. They aim merely to generalize the uniformities of sequence observed in nature.

In any science it is quite legitimate to suggest a working hypothesis which goes beyond the facts and reconstructs them. The electron theory and Mendeleeff's periodic law are such reconstructions of physical and chemical data. So in psychology Professor Münsterberg may find grounds for his theory of "self as a system of purposes." But such a theory should be based on scientific foundations. Instead of two standpoints, the causal and purposive, we should have systematic description of mental phenomena and a suggested reconstruction; the latter should amplify the empirical laws, instead of rejecting them.

The author's attempt to formulate a system of psychology from the teleological standpoint will not appeal to the plain empirical psychologist, because it runs counter to the scientific development of the subject. It transcends the scientific limitations of both cause and purpose. "Necessity" is an anthropomorphic addition to causality. Failing to find any such necessary connection between mental events, the author throws his science to the winds and bases his teleological reconstruction on an equally anthropomorphic interpretation of purpose. The result is perplexing. It is not easy to attach a definite meaning to such statements as "the free act is free because it has no causes" (324). Nor can we take a definite attitude toward the assertion that "we can not imagine a purposive act the meaning of which is not a negation of an opposite purpose" (316).

In the third part of the volume Professor Münsterberg returns to more familiar terri-

tory and discusses the applications of psychology to science and art. He indicates the line of demarcation between psychology and the human sciences as follows: The understanding of mental operations is valuable in the study of history, sociology, etc., but the interpretation of the subject-matter in each case belongs to the special science and not to psychology. In his closing chapters the author considers the applications of psychological data and methods to education, law, economics, medicine and culture. To this applied field he gives the name psychotechnics. These chapters offer a most interesting presentation of the recent progress in applied psychology, a line of development which seems likely to bring about a closer connection between psychology and the professions.

Whether or not the reader agrees with Professor Münsterberg's fundamental positions, he will find the present work most stimulating and suggestive.

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Design of Polyphase Generators and Motors. By HENRY M. HOBART. McGraw-Hill Book Company.

In "Design of Polyphase Generators and Motors," Mr. Hobart takes up the design of a simple three-phase generator and an induction motor from the standpoint of a designing engineer. This occupies the major portion of the book, but there are in addition two chapters devoted to a comparison of synchronous motors and induction motors and to the induction generator. Much useful information and many valuable tables compiled from empirical data obtained from existing machines are included.

The book follows the plan, outlined by the author in its preface, of taking up immediately without any preliminary discussion the design of a three-phase generator of definite rating, introducing the principles involved when required as the design progresses. In addition to the design of a three-phase generator, the design of a polyphase induction motor is also considered. The book should be valuable to the young designer who has a fair knowledge of the principles underlying operation and design of electrical machinery.

It is to be regretted that a portion of the book is not devoted to a simple analytical study of the effect on the operating characteristics of machines of modifying their dimensions and windings in order that the young designer might learn to analyze existing designs and to be able to judge the fitness of any particular design for a definite class of service.

Two appendices give a full bibliography of the papers dealing with polyphase generators and motors which have been printed in the *Proceedings* of the American Institute of Electrical Engineers and in the *Journal* of the British Institute of Electrical Engineers.

RALPH R. LAWRENCE

Synchronous Motors and Converters. By ANDRÉ BLONDEL. Translated from the French by C. O. MAILLOUX. McGraw Hill Book Co. 1913.

"Synchronous Motors and Converters" is a translation of the admirable little book by André Blondel entitled "Moteurs Synchrone à Currents Alternatifs." Several chapters have been added to the translation in order to increase the scope of the book and to bring it up to date. The translation is divided into three parts. Part I. is a translation of the original book with one chapter added by Professor C. A. Adams, of Harvard University. Part II. relates to Rotary Converters and is made of new material by Professor Blondel and a translation of papers presented by him at the Paris Congress in 1900. Professor Adams has also added a chapter to this section relating to the split-pole converter. Part III. contains reprints of papers presented by Professor Blondel at the St. Louis Electrical Congress in 1904, relating to his "two reaction" method of treating the armature reaction of alternators.

The first part of the book takes up the general principles of synchronous motors and a study of their operation under different conditions, and is particularly valuable in giving the development of well-known Blondel bi-

polar circle diagram. In translating Professor Blondel's "Moteurs Synchrone," Mr. Mailloux has rendered a valuable service to English-speaking electrical engineers.

Ralph R. Lawrence

Storage Batteries. By Harry W. Morse. New York, The Macmillan Company. 1912.

This little book of 263 pages on storage batteries is based upon lectures given by Professor Morse at Harvard University. It deals only with the theory and the characteristics of storage batteries. No attempt is made to discuss problems connected with storage-battery engineering. The first chapters are devoted to the laws underlying the action of storage cells and to the consideration of the fundamental reactions. A short discussion of the ionic theory and the energy relations involved in the action of a storage cell is included. Later chapters are given up to the operating characteristics, efficiency and capacity, and to the general principles underlying the methods of forming modern storage battery plates. The diseases and care of storage batteries are also discussed. In the last chapter a few pages are devoted to the iron-nickel-alkali cell. "Storage Batteries" is an excellent little book for any one who wishes a simple treatment of the theory, action and care of lead-lead-peroxide storage batteries.

Ralph R. Lawrence

SPECIAL ARTICLES

CORRELATION BETWEEN EGG-LAYING ACTIVITY AND YELLOW PIGMENT IN THE DOMESTIC FOWL[1]

In the Leghorns and the so-called American breeds, such as the Plymouth Rocks, yellow, in the form of yellow fat,[2] is present in varying amounts in the legs and beak. In these breeds, individual birds may undergo considerable change in the amount of the yellow pigment visible. The paling or yellowing of the legs has been attributed by poultrymen to various environmental factors. Of recent years, some individual poultrymen, however, have claimed that paling of the legs is due to heavy laying.[3] The requirements of the "Standard of Perfection," which controls judges in the show room, as well as the common practise of poultry breeders, are opposed to a belief in any connection between laying and leg color. Woods[4] under the title, "Has Leg Color Value Indicating Layers?" in the most recent discussion of the subject, concludes:

> Personally we believe that, as a practical guide in the selection of heavy layers, . . . the leg color of itself has no real value.

So far as the writers are aware, no published data are available which show in how far the leg color may be of any value in selecting the laying hen, and such suggestions as have been made in this connection have confined themselves almost entirely to a consideration of the legs alone. The results tabulated in the present paper show conclusively, it is believed, that a close connection does in fact exist between the yellow pigmentation in a hen and her previous egg-laying activity, and that, in Leghorns, the color of the ear-lobes is perhaps a better criterion of laying activity than either legs or beak and is more readily recorded.

The hens investigated were in the egg-laying contest at Storrs, Conn., and were handled essentially alike. The influence of environmental factors, therefore, can be largely neglected. The amount of yellow was measured by means of the Milton Bradley color top, which, when spinning, acts as a color mixer. The top readings were taken of the White Leghorns listed in Tables I. and II. at three different periods in October.

In Table I., the records at the three different readings have been used. A bird laying on the day of record, or on a later day within the month is considered to be laying and credited

[1] Paper presented before the American Society of Naturalists, Philadelphia, December 31, 1914.

[2] Barrows, H. R., "Histological Basis of Shank Colors in Domestic Fowl," Bull. 232, Maine Agric. Exper. Station, 1914.

[3] Rice, J. E., Circular 11, p. 42, N. Y. State Dept. of Agriculture, 1910; Barron, Tom, *Connecticut Farmer,* September 12, 1914; Circular 499, Maine Agric. Exper. Station. This is listed as an abstract of Bull. 232.

[4] Woods, P. T., *Amer. Poultry Jour.,* p. 35, January, 1915.

TABLE I

Percentage of Hens Laying and Average Number of Days since Laying for Different Amounts of Yellow in Ear-lobes

Per Cent. Yellow	5–10	11–15	16–20	21–25	26–30	31–35	36–40	41–45	46–50	51–55	56–60	61–65	66–70	71–75
No. records	41	125	80	67	62	92	94	94	108	84	44	28	9	4
Av. days since laying	0.4	1.6	7.3	17.1	26.2	37.9	41.5	44.0	45.1	51.3	55.9	61.4	50.3	71.0
No. records = laying	36	98	44	17	3	0	1	0	2	0	0	0	0	0
Per cent. records = laying	87.8	78.4	55.0	25.4	04.8	0	01.0	0	01.9	0	0	0	0	0

White Leghorns. Total number of records, 932; total number of birds, 317

with a zero. If she laid on the day before the record but not later, she is credited with one "day since laying" and in a similar way a longer period of inactivity in laying is indicated by a larger number of days since laying. With the exception of a few cases where this is not possible three records were taken of each bird. Since October is the season of decreasing egg production, the majority of the birds increased their quantum of yellow and consequently most birds are listed in more than a single color grade. Beginning with the 41 records in the 5–10 per cent. color grade, which show an average of only 0.4 days since laying, the number of days increases consistently with the amount of yellow in the ear-lobes. The percentage of records that indicate actual laying drops rapidly from 87.8 per cent. for 5–10 per cent. yellow to zero for grades of yellow above 30 per cent. The three cases of laying among records above 30 per cent. yellow were for sporadic layers. The table shows that it is practically certain that a bird with an ear-lobe showing more than 30 per cent. yellow at the time of the records is not in a laying condition.

Table II. shows the percentage of yellow in the ear-lobes of 312 birds according to the color records of October 20, together with egg records for the different color groups. It will be seen that in general as the percentage of yellow increases the egg production falls off, and that the correlation is most marked during the periods nearest the time when the records were taken. A distinct correlation with color seems to show in the yearly averages but is largely an indirect one. It is generally only the best birds—those that make the large yearly records—that are laying in October. Therefore, any method that selects the laying birds at this season will select at the same time the birds laying above average throughout the year, and consequently give high yearly totals. It will be observed that 30 per cent. seems to be a critical amount of yellow. Above this amount comes the sudden drop in egg production for the months of September and October and also above 30 per cent. yellow the yearly totals fall to between 130 and 140 with but slight change thereafter.

TABLE II

Average Egg Records for Different Amounts of Yellow in Ear-lobes of 312 White Leghorns

Per Cent. Yellow	5–10	11–15	16–20	21–25	26–30	31–35	36–40	41–45	46–50	51–55	56–60	61–65	66–70	71–75
No. birds	7	36	40	16	20	31	33	41	39	30	13	4	1	1
September	19.7	18.2	16.9	16.4	10.3	5.5	6.1	4.9	4.0	3.6	2.4	1.3	0.0	0.0
October	15.3	14.2	11.7	8.1	3.2	0.5	0.2	0.2	0.2	0.1	0.0	0.3	0.0	0.0
Year	197.1	187.9	184.3	164.3	148.5	139.1	139.6	134.2	138.2	137.8	124.7	100.8	70.0	83.0

By the use of beak and leg color, similar results to those shown in Tables I. and II. have been worked out for other breeds than Leghorns and more complete data are being published elsewhere.

The data presented indicate a connection between the amount of yellow pigment showing in a hen and her previous laying activity. The most natural assumption is that laying removes yellow pigment with the yolks more

rapidly than it can be replaced by the normal metabolism, and in consequence, the ear-lobes, the beak and the legs become pale by this subtraction of pigment.

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Proceedings of the Annual Meeting of the American Society of Zoologists Held in Philadelphia 1914

The American Society of Zoologists, in conjunction with the American Society of Naturalists and Section F of the American Association for the Advancement of Science, held its twelfth annual meeting (the twenty-fifth annual meeting of the society since its establishment as the American Morphological Society) in the zoological laboratory of the University of Pennsylvania, Philadelphia, Pennsylvania, on December 29 and 30, 1914.

At the session for transacting business, held on the afternoon of December 30, the following officers for the society were elected for the year 1915:

President—William A. Locy, Northwestern University, Evanston, Ill.

Vice-president—William E. Ritter, Scripps Institution, La Jolla, Cal.

Member at large of the Executive Committee—D. H. Tennent, Bryn Mawr College, Bryn Mawr, Pa.

Upon the recommendation of the executive committee the following persons were elected to membership in the society:

Cora J. Beckwith, assistant professor of zoology, Vassar College; Ralph V. Chamberlain, museum of comparative anatomy, Harvard University; Margaret H. Cook, instructor in zoology, Wellesley College; J. A. Detlefsen, assistant professor of genetics, University of Illinois; Howard E. Enders, associate professor of zoology, Purdue University; Nathan Fasten, instructor in zoology, University of Washington; Richard B. Goldschmidt, in charge of department of genetics, Kaiser Wilhelm Institut für Biologie, Berlin (Yale University); Joseph Grinnell, director, museum of vertebrate zoology, University of California; Carl G. Hartman, adjunct professor of zoology, University of Texas; Mildred A. Hoge, instructor in zoology, Indiana University; A. G. Huntsman, lecturer in biology, University of Toronto; B. F. Kingsbury, professor of histology and embryology, Cornell University; F. H. Krecker, assistant professor of zoology, Ohio State University; K. S. Lashley, Adam T. Bruce Fellow, Johns Hopkins University; W. H. Longley, professor of botany, Goucher College; Elmer J. Lund, instructor in zoology, University of Pennsylvania; Roy L. Moodie, instructor in anatomy, University of Illinois; Julia E. Moody, instructor in zoology, Wellesley College; Anna H. Morgan, associate professor of zoology, Mount Holyoke College; T. S. Painter, instructor in biology, Yale University; B. M. Patten, instructor in histology and embryology, Western Reserve Medical School; B. H. Ransom, chief, zoological division, Bureau of Animal Industry, Washington, D. C.; E. E. Reinke, instructor in zoology, Rice Institute, Houston, Texas; Lucy W. Smith, instructor in zoology, Mount Holyoke College; A. H. Sturtevant, Cutting Fellow, Columbia University; Shiro Tashiro, instructor in physiological chemistry, University of Chicago; Ernest I. Werber, assistant in biology, Princeton University; Paul S. Welch, assistant professor of entomology, Kansas State Agricultural College.

The secretary-treasurer of the society was authorized to prepare and print a list of the names, addresses, etc., of the members and officers elected at this meeting and any corrections or additions needed to be made to the published list of members, and to distribute copies of the same to all members. He was also instructed to secure and distribute to members reprints of the proceedings of the Philadelphia meetings when the same shall have been published in Science.

The committee on premedical education, appointed at the last annual meeting, submitted no report and it was continued with instructions to report at the annual meeting in 1915.

The executive committee, to which the "Matthews Plan for the Organization of an American Biological Society" was referred last year for consideration and report to a future meeting, asked and was granted more time for this work.

The question of holding a mid-year meeting of the society, as a whole, in San Francisco in connection with the Panama Exposition was considered and, upon motion by Professor R. G. Harrison, the society took the following action: "The American Society of Zoologists urges its members who reside on the Pacific coast to form a section of the society, such as is provided for by the constitution, and that this section cooperate in organizing and holding a zoological meeting in San Francisco in connection with the Panama Exposition, and it assures these members of the sincere interest and approval of the society in such an undertaking."

A committee on resolutions on the death of Professor Charles Sedgwick Minot and Professor Seth Eugene Meek, consisting of Professors Frank R. Lillie, R. G. Harrison and H. V. Neal, was appointed and instructed to prepare resolutions and publish the same in Science and to transmit copies to the families of the deceased members.

The secretary-treasurer submitted the following financial statement which, having been examined and found correct by the auditing committee, consisting of Professors A. S. Pearse and R. A. Budington, was accepted by the society.

Receipts

1914			
Jan.	1	Balance on hand (Eastern Branch)	$361.71
	18	Received from Treasurer Central Branch	235.51
		Received during the year annual dues from members	247.03
Feb.	14	Received first dividend from the Permanent Fund from the Custodian, J. H. Gerould	22.50
Oct.	13	Received interest on Current Funds on deposit with the Title, Guarantee and Trust Co., Baltimore	23.58
Nov.	11	Received second dividend from the Permanent Fund from Custodian, J. H. Gerould	15.00
		Total receipts	$905.33

Expenditures

Jan.	3	for "Smoker" supplies	$19.00
	14	for circular letter to new members	1.40
	22	for typewriting by-laws for executive committee	1.75
	26	for stamps for mailing the above.	.50
	26	for blanks forms for addresses, etc., of members	1.15
	28	for express on MSS. of Proceedings to Science	.25
	29	for typewriting circular letter to executive committee	.60
	29	for express on "files" from Secretary, Central Branch	1.00
Feb.	6	for 1,000 special stamped (2 c.) envelopes	21.36
	11	for I. P. Binder and two packages *Journal* sheets	2.25
	28	for stamps	1.50
Mch.	31	for addressing and mailing due bills and circular letters	2.55
Apr.	13	for new journal	.50
May	30	for multigraphing blank forms.	1.50
June	6	for typewriting constitution, by-laws, list of members	5.85
	17	for express on files, typewriter and MSS. to Woods Hole	1.60
Sept.	17	for express on MSS. of List of Members to printer	.22
Oct.	1	for express of files and typewriter, Woods Hole to Baltimore	1.60
	24	for 500 copies printed constitution, by-laws, list of members, etc.	75.00
	26	for 500 Columbia Clasp envelopes.	3.50
	26	for 500 copies printed blanks for nominations of new members.	2.75
	26	for mailing copies of Constitution, etc.	7.46
Nov.	4	for 300 copies announcement of Philadelphia meeting	3.25
	4	for mailing announcements to members	1.50
Dec.	4	for 300 copies "Preliminary Program"	18.10
	12	for mailing preliminary programs and map to members.	3.85
	18	for 280 special stamped (1 c.) envelopes	3.09
	26	for 500 printed programs for the annual meeting	10.40
	26	for 500 sheets typewriter paper.	1.90
	28	for R.R. fare of secretary to Philadelphia and return	4.80
	30	for expense incurred by the Secretary in attending the annual meeting	16.00
		Total expenditures	$216.18
		Total receipts	$905.33
Dec.	30	Balance on hand	$689.15

At sessions held during the forenoons and afternoons of December 29 and 30 the following papers were read either in full or by title:

In order to complete the program by the end of the fourth session and thus clear the way for adjournment to attend the session of the Naturalists scheduled for the forenoon of December 31, it was found necessary to provide for the simultaneous meeting of two sections of the Zoologists during the afternoon of December 30. At one of these sectional meetings papers grouped under *General Physiology* and some under *Miscellaneous* were read, and those under *Ecology* and the remaining *Miscellaneous* papers were read at the other. For the same reason practically no time was taken at any session for the discussion of facts and conclusions presented.

Comparative Anatomy

Nerve and Plasmodesma: H. V. NEAL. (With lantern.)

The present paper, based upon observations upon *Squalus* embryos preserved by the Bielchowsky-Paton method, attempts to give an answer to three controverted problems in nerve histogenesis:

1. Are connections between tube and myotome primary or secondary?

2. Are neuromuscular connections primarily undifferentiated plasmodesmata or are they primarily neurofibrillar?

3. Are neuromuscular connections effected by indifferent—neurilemma—cells or by medullary neuroblasts?

The answers given to these questions are:

1. Previous to the stage of 4.5 mm. there are no protoplasmic connections between tube and myotome in *Squalus* embryos.

2. Neurofibrillar substance is present in the first protoplasmic connections between tube and myotome. In the primary protoplasmic connections appear deeply staining neurofibrils which may be traced to bipolar neuroblasts within the neural tube. The claim that the primary connections consist of undifferentiated plasmodesmata therefore is based upon inadequate neurological methods.

3. In stages before protoplasmic connection between tube and myotome is effected certain medullary cells in zones where later the nerve anlagen make their appearance show in Bielchowsky-Paton preparations a deeply-staining neuro-reticulum. In slightly later stages when neuro-muscular connection is established similar neuro-reticular cells are found connected with neurofibrillæ extending into the nerve anlagen in the manner characteristic of medullary neuroblasts stained by specific neuro-fibrillar stains. The evidence of the presence of similar neurofibrillar substance in all parts of the nerve anlagen supports the inference that the neuromuscular connections are established—not by indifferent cells—but by medullary neuroblasts, as maintained by supporters of the Bidder-Kupffer theory. Indifferent cells participate in the formation of nerve anlagen only in more advanced stages by a process of migration from the neural tube.

The Components of the Fenestral Plate in Necturus: H. D. Reed.

In previous communications it has been pointed out that in certain urodele families the sound-transmitting apparatus consists of a single piece resulting from gradual growth during larval and early adult life. In such forms the plate is compound. The stylus represents columella or the extraotic element, while the plate itself arises from chondrification within the fenestral membrane and therefore otic in nature.

The fenestral plate in *Necturus* has been considered as *columella*. It arises outside the ear capsule and gradually comes to lie against the fenestral membrane over the cephalic portion of which it spreads through growth eventually filling the fenestra at this level. Caudad the plate tapers coming to a point at about the middle of the fenestra. The plate thus formed is soon encased in bone. About the margin of this triangular columellar plate cartilage is formed by chondroblasts which arise in the fenestral membrane. The matrix which is soon secreted is invaded by bone deposited in continuity with the previously formed bony case. Thus cell by cell the definitive structure is completed by additions to the margin of the columellar plate. The fenestral plate is to be considered, therefore, as a compound structure possessing both otic and extraotic elements and must be looked upon as a morphologic intermediate of the condition found in *Ambystoma* and the Plethodontidæ.

Variations in the Rays of Ten Thousand Star-Fish, Asterias Forbesii: Franklin D. Barker. (With lantern.)

A New Dignetic Trematode from the Crayfish: John W. Scott.

In 1827 Von Baer described a fluke from the crayfish to which he gave the name *Distomum cirrigerum*. Warren ('03) described its anatomy and development, and Sulowiow ('11) discussed its structure and systematic position. During the past two years trematodes from American crayfish have been secured; these are all encysted, sexually mature, individuals. In certain points they are quite similar to the European form, but the differences are so striking as to place them in different species. Wright ('84) appears to have been the first to observe the fluke in this country, but mistook it for *D. nodulosum*. Linton ('92) gives a brief description of immature specimens, and calls attention to Wright's mistake. The American species differs from the European in the following particulars. It has no conical or plate-like cuticular scales; it has two lateral palp-like extensions of the oral sucker, and four papillæ; the esophagus is short, the gastric coeca arising in front of the genital pit; the yolk glands extend nearer the anterior end of the body; both testes are median, or nearly so, and one lies in front of the other; small cuticular denticles are found on the oral sucker; the cerebral ganglia are wider apart, the prostate gland better developed, and the excretory bladder of somewhat different shape.

A full description of the new form, with a discussion of its probable systematic relationship, will soon be published.

The Reflex "Bleeding" of the Coccinellid Beetle, Epilachna Borealis: N. E. McIndoo.

The Gland of the Clasper in Sharks: E. W. Gudger.

In the claspers of sharks, on the inner and dorsal surface the tissues are modified to form a

groove. This is continued forward between the skin and the pelvic arch where it enlarges to form a sac, which in its turn is extended forward as a tube between the skin and the belly wall. Each tube ends blindly near the median line, in some extending nearly to the pectoral girdle. The function of this organ is entirely unknown.

These structures were described from *Hypoprion brevirostris* and *signatus*, and from the tiger shark, *Galeocerdo tigrinus* captured at Key West and at Tortugas, Florida. The longest gland (one foot, 7¾ inches) was found in a 4 foot, 10 inch specimen of *H. signatus*.

The scanty literature of this organ from its discovery by Andrew Smith in 1849 was briefly sketched. The full data will be given later in an article in "Papers from the Tortugas Laboratory," published by the Carnegie Institution of Washington.

Pre-Otic Somites in Cyclostomes: H. V. NEAL. (With lantern.)

Concerning no other criterion of the metamerism of the vertebrate head do observations so fully agree as with regard to the mesodermic divisions discovered by Van Wijhe ('82) in Selachian embryos. His discovery has been confirmed by Miss Platt ('91), Hoffman ('94), Neal ('96), Sewertzoff ('98), Braus ('99) and Johnston ('09). Moreover, Sewertzoff showed that the more numerous "microcœlic" divisions described by Dohrn ('90) and Killian ('91) in *Torpedo* embryos secondarily unite to form the somites of Van Wijhe. Furthermore, a mesodermic segmentation which may be compared with that of Elasmobranchs has been discovered by Miss Platt ('97) in Amphibia and by Koltzoff ('02) in Cyclostomes.

The mesodermic segmentation discovered by Koltzoff ('01) in *Petromyzon* embryos is especially significant and important, since in this animal according to Koltzoff the segmentation of the head mesoderm is complete as in *Amphioxus* and the somites develop as dorso-lateral diverticula of the endoderm. Thus *Petromyzon* is in this respect as in others intermediate between *Acrania* and *Gnathostomata*. Koltzoff finds that the three anteriormost somites give rise to the eye muscles as they do in the Elasmobranchs.

The importance of the evidence as bearing on the past history of the vertebrate head has led me to examine sections of *Petromyzon* embryos in the hope of confirming Koltzoff's results. In at least two series of eight-day *Petromyzon planeri* embryos the evidence presented seems to bear out Koltzoff's contention that the pre-otic segmentation of the mesoderm is comparable with that of Elasmobranch embryos. The anterior head mesoderm is completely segmented as Koltzoff has asserted. No homologue of Miss Platt's "anterior" somites, however, is present in the Cyclostome.

The Absence of Male Reproductive Organs in Trematodes: FRANKLIN D. BARKER. (Lantern slides and demonstrations.)

Does Amphioxus Eat with His Left Ear?: H. V. NEAL. (With lantern.)

It was Van Wijhe ('93) who first suggested the homology of the larval mouth of *Amphioxus* with the left spiracle of Selachians and asserted that "Amphioxus can not hear; he eats with his left ear and consequently has lost his mouth." The homologue of the craniote mouth in *Amphioxus* is, according to Van Wijhe, the pre-oral pit.

The present paper raises the problem: Are we to accept the homology of the mouth of *Amphioxus* with the spiracular cleft of *Craniotes?*

The homology suggested by Van Wijhe is based on the following grounds:

1. The mouth of *Amphioxus* is an organ of the left side as evidenced by its development, its left-sided innervation and its topographic relations to the club-shaped gland, which Van Wijhe regards as the antimeric gill-pouch.

2. The relations of the larval mouth of *Amphioxus* to the second mesodermic cavity and to the splanchnic muscles derived from it are similar to those of the left spiracle of *Craniotes* to the second mesodermic head-cavity.

Van Wijhe's homology may not be accepted on the following grounds:

1. Since all median openings of *Amphioxus* are asymmetrically displaced, the left-sided position of the mouth is not significant.

2. The left-sided innervation is likewise indecisive. If the homology suggested by Van Wijhe were the correct one, the velum should be innervated by the left nerve of the third pair. It is actually innervated by the left nerves of the 4–7 pairs. Primary nerve relations are obviously disturbed and inferences from innervation precarious.

3. The club-shaped gland and its duct represents a pair of gill-pouches and not a gill-pouch of the right side only, as Van Wijhe's homology would require. The club-shaped gland represents the second pair of gill-pouches and the endostyle anlage the first pair of gill-pouches of *Amphioxus*.

4. The chief objection to the homology maintained by Van Wijhe is the fact that the anterior endodermic diverticula of *Amphioxus* are homologous—not with the pre-mandibular head cavities of Elasmobranchs as assumed by Van Wijhe—but with the "anterior" head cavities. The homologues of the pre-mandibular cavities of Elasmobranchs (the first permanent myotomes) are the first permanent myotomes of *Amphioxus*.

Amphioxus does not eat with his left ear. The homologue of the left spiracle is the first transient gill cleft of *Amphioxus*. The mouth of *Amphioxus*, however, is not homologous with the mouth of *Craniotes*. If it is homologous with any organ of the *Craniotes*, that organ is the hypophysis.

Embryology

Internal Factors Producing the Swarming of the Atlantic Palolo: Aaron L. Treadwell.

Previous explanations of the swarming of annelids have been based on the influence of the external factors such as light, tidal pressure, etc. At the Carnegie Laboratory in the Dry Tortugas I was able with the cooperation of Dr. Tashiro to test the hypothesis that an internal factor cooperates in producing this effect. Since all the eggs of the Atlantic palolo are laid at one definite time, it is possible to test the eggs at any desired interval before the time when they would normally be laid. Testing with his biometer, Dr. Tashiro found that five days before laying each egg gave off 0.000,000,07 grams of CO_2 per minute; two days before laying 0.000,000,083 grams, while eggs taken from the body of a swarming female were eliminating 0.000,000,13 grams per minute. All eggs were taken from the body without mixture with sea water. This indicates an increase in metabolic activity as the time of swarming approaches, and the conclusion follows that this furnishes an internal stimulus of importance in producing the swarm. Probably a similar stimulus is operative in ordinary egg laying.

Are the Taste-buds of Elasmobranchs Endodermal in Origin?: Margaret H. Cook. (Introduced by H. V. Neal.)

An attempt to determine the origin, whether ectodermal or endodermal, of taste-buds in *Squalus acanthias*. A study was made of sections of embryos of 7 to 80 mm. supplemented by dissections of "pup" and adult stages.

Taste-buds in this species are limited to the region of the pharynx, which in all stages of ontogenesis is lined with endoderm. No marked encroachment of the ectoderm is perceptible even in the mouth region.

Scales similar to those which characterize the outer skin arise in late stages of ontogenesis in both the floor and roof of the pharynx. Thus two kinds of organs usually classed as ectodermal, viz., taste-buds and placoid scales, appear to arise from the endoderm of the pharynx of *Squalus acanthias*. To assert that the pharyngeal taste-buds and scales of *Squalus* are ectodermal would necessitate the assumption that the endodermic lining of the pharynx completely disappears during ontogeny and is replaced by ectoderm. Evidence of such substitution is wholly lacking.

These results extend to the Elasmobranchs the conclusion of Johnston ('98 and '10) that the taste-buds of Teleosts and Amphibia are derived from endoderm. They also add to the structures derived from the endoderm the pharyngeal scales which have hitherto been assumed to be ectodermal, and thus add another exception to the law of the specificity of the germ-layers.

On the Larval and Post-larval Development of the Coral, Agaricia Fragilis, Dana: J. W. Mavor. (With lantern.) (Introduced by E. L. Mark.)

Tissue and Organ; Their Rôles in Morphogenesis: Herbert W. Rand.

Definiteness of form is the essential characteristic of an organ. Tissue is without form. Our attempt to discover the factors immediately responsible for the form of an organism will be furthered if we clearly distinguish the parts played by organic units of the several grades. How far does a given formative event depend upon cells acting as uncoordinated individuals, how far does it depend upon a system of cells coordinated into a tissue, and how far does it exhibit the impress of organization higher than that of tissue?

In the wound-closing activities of tentacles of actinians, cells, as such, play a minor and probably unessential part. The definitive structural closure is an autonomous tissue process. Accompanying activities of the neuro-muscular complex afford temporary protection and favor the carrying out of the tissue process. In these activities and in other reactions of the neuro-muscular complex, we observe polarity and a variety of definite relations to the form of the organism. These bespeak for the neuro-muscular complex a degree of organization higher than that of mere tissue, probably corresponding to the organism as a whole.

The neuro-muscular complex may be regarded as standing at a threshold of organization. Morphologically standing at the level of a tissue, it exhibits the physiological definiteness and differentiation which characterize an organ. Thus, in a sense, function anticipates structure.

(Based upon a paper now in press in *Archiv für Entwickelungsmechanik der Organismen.*)

The Form of the Stomach in Embryos of the Cat, Albino Rat, Pig and Sheep: CHESTER H. HEUSER.

Utero-gestation in the Sheep-nosed Shark, Scoliodon Terranovæ: E. W. GUDGER.

In a 37½-inch specimen with a girth of 13½ inches taken at Tortugas, Florida, the left ovary was twice as long as the non-functional left lobe, while the oviducal apparatus was paired, symmetrical, and had both sides functional. The eggs, each enclosed in a thin yellow shell with its long pointed ends curiously folded and plaited, lay in crypt-like lateral "nests" formed in the mucous lining of the uteri. The structure of the uterus and the formation of the "nests," with the relation thereto of the curious shells, were described and illustrated, as were also the young and their connection with the yolk and finally with the uterine wall. In all respects the eggs and their shells together with the uteri containing them are in close parallel with similar structures in the bonnet-head shark, *Sphyrna tiburo,* reported on by the speaker at the Princeton meeting of the society in 1911.

An article giving all the data at hand and illustrated by photographs will appear later in "Papers from the Tortugas Laboratory" of the Carnegie Institution of Washington.

Experimentally Fused Larvæ, with Special Reference to Changes in Polarity, Symmetry, Synchronity, Etc.: A. J. GOLDFARB.

Experiments in Cleavage: T. S. PAINTER. (Introduced by R. G. Harrison.)

Cytology

A Study of the Maturation Period in the American and European Molecrickets: F. PAYNE.

Regenerative Potencies of Dissociated Cells of Hydromedusæ: CHARLES W. HARGITT.

The paper describes experiments made at the Zoological Station, Naples, several years ago. About a dozen species of hydroids, and one species of medusa were experimented on, and with results which in the main confirm those of H. V. Wilson, published since my own were made. The paper also briefly reviews a series of similar experiments made by DeMorgan and Drew.[1] These latter experiments are the immediate occasion for giving publicity to my own work, as they appear to imply some doubt as to the conclusiveness of Wilson's work. Their experiments were made upon two species of *Antennularia,* and while serving to confirm earlier phases of those of Wilson they never gave rise to new hydranths. The authors declare "our experiments have resulted in the production of masses that are certainly abnormal and pathological, but nevertheless we would submit that the segregation and rearrangement of the cells after isolation, and the considerably long duration of life of the tumor-like masses to which they give rise, are facts of considerable theoretical interest."

The paper will show that the assumption as to the abnormality and pathological conditions apparent are not warranted by the more extended knowledge of facts from these and other sources. Indeed, many facts concerning the behavior of these organisms in development and regeneration seem to prove that fundamentally there is neither abnormality nor pathological process involved.

Microdissection Studies on the Physical Properties and Behavior of Cell Structures, Especially in Orthopteran Spermatogenesis: ROBERT CHAMBERS, JR.

Cells studied were of Orthopteran gonads, plant-root tips and pancreas of frog. Fresh material corroborates in many interesting details nuclear structures observed in fixed material. Mitochondria and the cytoplasm, however, largely show artifacts with fixatives.

Puncture of a cell by a needle generally causes irreparable injury. Slight injury hastens the normal reversible changes in the physical states of the colloids in the cell, but soon transforms them to an abnormal condition which leads to death.

Injury to the cell is followed by swelling accompanied by an increased imbibition of water.

Physiological salt solutions are more or less injurious to cells normally bathed in organic fluids.

A tension exists in the cell during division which is lost when any part of the cell is torn.

Janus green (Hoechst) stains mitochondria rapidly. In the nucleus it is reduced to safranin, which kills the cell.

[1] *Jour. Marine Biol. Assoc.,* Plymouth, October, 1914.

Janus green produces coagulation phenomena in living protoplasm and therefore should not be used to identify mitochondria.

The mitochondria are rigid structures. In the Orthopteran germ they all change from granules to strands, they coalesce, they disappear and reappear and may be expressions of changes in the physical states of the cytoplasmic colloids.

The chromosomes behave almost as do the mitochondria. In the hyaline resting nucleus they appear in the form of granules ranged about a hyaline resistant core. The granules coalesce to form the homogeneous body of the metaphase chromosomes. In telophase the chromosomes swell and disappear. Some internal chemical condition may exist which so regulates the physical states of the nuclear colloids that a constant number of chromosomes periodically appears.

Spermatogenesis in Paratettix: Mary T. Harman.

1. The chromosomal complex of the spermatogonial divisions of *Paratettix leuconotus—leucothorax* consists of thirteen rod-shaped chromosomes which may be divided into two groups, one consisting of four large chromosomes and the other of nine smaller ones.

2. Eight of the smaller chromosomes are straight rods; one of the smaller ones and all of the larger ones are U-shaped. The chromosomes do not form equal pairs.

3. In the metaphase stage the chromosomes are at right angles to the spindle fibers, but in the anaphase they are parallel to them.

4. One chromosome is always far to the center of the spindle. Sometimes it is completely surrounded by the others and sometimes merely one end is at the center of the spindle. It is never the bent chromosome but is always one of the larger ones of the group of nine.

5. In the early prophases is always a mass of chromatin which never takes on the reticular condition, but has a more compact consistency and stains more intensely than the remainder of the chromatin material.

6. At the beginning of the growth period the nucleus becomes large, and some of the chromatin takes on the reticular condition and stains lightly, but there is one mass that is compact, stains intensely and has the appearance of a nucleolus. It forms the accessory chromosome.

7. In synizesis there is no polarization of the chromatin thread.

8. In the primary spermatocyte are always six dumb-bell-shaped chromosomes but two are much larger than the others.

9. The first spermatocyte division is always a cross division. The accessory chromosome always lies near the periphery of the spindle and passes to one pole undivided much in advance of the others.

10. All the chromosomes divide in the second spermatocyte division.

Synapsis and the Individuality of the Chromosomes: D. H. Wenrich.

In attempting to determine whether synapsis in this Acridid grasshopper is end-to-end (telosynapsis) or side-by-side (parasynapsis), it was found that the only method by which conclusive evidence could be obtained was that of following the history of individual chromosomes.

Of the 12 haploid chromosomes present in this species, at least three were found to possess individual peculiarities by which they could be recognized throughout the growth period and the prophases of the first maturation division.

Parallel conjugation of the fine spireme threads of the early growth stages appeared to occur as a general rule, and different steps in the process could be followed for at least one of the differential chromosomes. Conjugation did not result in loss of identity of the uniting threads in the sense of forming "mixochromosomes," for the plane of separation between them remained visible throughout the spireme stages. However, pairs of granules often appeared to be fused into single ones. Spireme segments separate out as rods or loops with a single split, tetrads being formed by a second longitudinal split at right angles to the one already present.

Analysis of the spireme stages of one of the differential chromosomes revealed a seriation of granules (chromosomes) along its length, such that the relative size and position of the granules were constant not only in the cells of one individual, but in those of all the animals studied.

Chromosomes with peculiarities analogous to those found in the first spermatocyte could be recognized in the spermatogonia.

In the first maturation division the monosome passes to one pole undivided. The tetrads appear to divide equationally with one exception. In this tetrad the conjugants are very unequal and division is as often reductional as equational. When dividing reductionally the unequal dyads show, with reference to the monosome, a distribution according to the law of chance (Mendel's law).

The Orientation of the Nuclear Contents in the Motor Electric Cells of Torpedos: ULRIC DAHLGREN. (With lantern.)

The nucleus contains, besides the usual chromatin bodies, a large typical plasmosome and a somewhat smaller body, the para-nucleolus. These two are always oriented in a dorso-ventral position and the cause of this orientation was sought in either the electric current that passes through the tissue or in the influence of gravity. Electric currents of the same strength as those generated by the fish and when applied at right angles to or directly against the orientation in question failed to influence it even when applied for several hours. Stronger currents moved the nuclear contents, but also changed the structure and chemical composition of the parts. One fact seemed to be shown; that the plasmosome was not moved to either pole of the nucleus, but assumed a position between two materials that did occupy the two halves of the nucleus.

Gravity experiments were interesting and seemed to solve the question; at a lower rate of centrifugal force the plasmosome was moved to the side of the nucleus away from the force. At a higher rate the chromatin bodies were also moved, while with the greatest force used the paranucleus was also moved. The Naples torpedos possess no paranucleolus in these cells.

Genetics

Bristle Inheritance in Drosophila: E. CARLETON MACDOWELL. (With lantern.)

A race of *Drosophila ampelophila* has been established from wild flies that has extra thoracic bristles. Crosses with normal flies prove that the extra bristled condition is a recessive Mendelian character. The number of extra bristles that appear in this race varies. The first six generations from parents selected for increase in bristle number showed a steady rise in the numbers of bristles. For thirteen generations after this, selection was apparently ineffective.

Three interpretations of successful selection may be examined.

1. Determiners may be inconstant; higher grades of a character have higher grade determiners. This would not account for the thirteen generations of ineffective selection, nor the genetic uniformity in the later generations which is evidenced by, (*a*) high and low grade parents from the same family giving like offspring, (*b*) analysis of high and low grades by crosses, (*c*) absence of correlation between means of parents and offspring in whole generations after the sixth.

2. Selection may produce a more vigorous line, and this vigor may occasion the better development of the character. A large fly in the *extra* race is apt to have more bristles than a small one.

3. Multiple factors may exist which are reduced to a homozygous condition by selection. Extracted extra bristles have a lower distribution than the uncrossed *extras,* yet the high extremes of the selected race are equalled. This would be the result if selection had removed some accessory restricting genes. These facts do not agree with the second interpretation, whereas all observations are in accord with the third interpretation.

The Behavior of a Unit Character in the Grouse Locust, Paratettix: ROBERT K. NABOURS.

Size Dimorphism in the Spermatozoa and Its Relation to the Chromosomes: CHARLES ZELENY AND E. C. FAUST.

Further evidence has been obtained in favor of the view that the size dimorphism of the spermatozoa observed in several species by the authors is correlated with the chromosomal dimorphism of the spermatids. The ratio between the chromosomal volumes was calculated from published figures of the spermatogenesis in the three species, *Musca domestica, Alydus pilosulus* and *Anasa tristis.* From this ratio the expected ratio between the head lengths in the resulting spermatozoa was calculated on the assumption that the size of the heads is directly proportional to the amount of chromatin received and on the further assumption that the shape of the heads is the same for all sizes. The calculated ratios and the corresponding observed ratios are as follows: *Alydus pilosulus,* calculated 1.00:1.06, observed 1.00:1.055; *Musca domestica,* calculated 1.00:1.08, observed 1.00:1.07; *Anasa tristis,* calculated 1.00:1.11, observed 1.00: 1.09. Complete data were given in the February, 1915, number of the *Journal of Experimental Zoology.*

Sex Controlled by Food Conditions in Hydatina Senta: DAVID D. WHITNEY.

The cause of the erratic proportion of the two sexes in *Hydatina senta* has been found to be due to diet. When three pedigreed parthenogenetic races of these rotifers were reared in the laboratory on a constant and uniform diet of a colorless flagellate, *Polytoma,* through 181–288 generations in 14–22 months they produced 96 per cent. to 100

per cent. of female grandchildren, thus showing that uniform food conditions cause nearly all females to be produced. However, when these rotifers that were producing almost exclusively female grandchildren on a uniform diet were suddenly put upon a new diet of the green flagellate, *Chlamydomonas*, they almost ceased producing female grandchildren and produced as high as 83 per cent. of male grandchildren. Moreover, if the first few eggs of each female that were laid in the *Chlamydomonas* diet were discarded all the grandchildren were males, thus showing that a sudden change from a uniform diet to a new diet causes the total suppression of nearly all females and the production of nearly all males.

Parthenogenesis and Sex in Anthothrips Verbasci: A. Franklin Shull.

The life cycle of few species of *Thysanoptera* is definitely and completely known. In general, sexual reproduction has been inferred in species having abundant males, especially if mating has been observed in nature. Such a species is *Anthothrips verbasci*, the mullein thrips. However, adult females reared in isolation from pupæ, and placed on thrips-free plants, have given rise to offspring. These offspring must have been produced parthenogenetically. It is not safe, therefore, to infer merely from the abundance of males or the occurrence of copulation, that *any* species is sexual. Whether *Anthothrips verbasci* exhibits both parthenogenetic and sexual reproduction has not yet been determined.

Twenty-eight of the parthenogenetically produced young have reached stages sufficiently advanced to allow their sex to be recognized. All were males. This suggests that the same relation exists between parthenogenesis and sex as in the honey bee and some other Hymenoptera, though other explanations are obviously possible.

Sex Control and Known Correlations in Pigeons: Oscar Riddle.

Some Internal Factors Influencing Egg Production in the Rhode Island Red Breeds of Domestic Fowl: H. D. Goodale.

Multiple Human Births: G. H. Parker.

A Note on the Origin of a Color Variety of Mice: Clarence C. Little.

A Modification of the Agouti Factor in a Cavy Species Cross: J. A. Detlefsen. (Introduced by W. E. Castle.)

The agouti character of the wild Brazilian cavy, *Cavia rufescens*, acts as a single unit in heredity, when transmitted to hybrids between this species and the tame species, *C. porcellus*. This unit character, however, is often modified in the hybrids. The modification is essentially a weakening in the power to restrict black and brown pigments from the sub-apical portion of the hair. The weakened modified agouti character of the hybrids was found to be a recessive in crosses with the normal agouti guinea-pig. The normal agouti of the tame guinea-pig, the modified agouti of the hybrids, and non-agouti, are triple-allelomorphs.

The Effects of Long-continued Parthenogenetic Reproduction (127 Generations) upon Daphnids: A. M. Banta.

The writer has kept pure lines of *Daphnia pulex* reproducing continuously by parthenogenesis alone for over three years. Some of the lines have now reached the 127th generation. If the sexual cycle is a necessary and essential feature of reproduction in this species the fact should ultimately become evident in the reduced vigor in the parthenogenetic lines. In order to discover if any reduction in vigor had actually occurred some "wild" *Daphnia pulex* were obtained from out-door ponds. These "wild" lines were treated in every way identically (except that no selection was made with them as with the older lines) with the lines already under observation. The age of the mother at the time her first brood appeared, the number of individuals in the first brood and the interval until a second brood was produced were taken as measures of the vigor of the individual. Average values obtained from large numbers of mothers of the "wild" lines and of the selection lines constituted the data finally obtained.

Measured by these standards, the lines reproducing parthenogenetically from the 70th to the 92d generation under laboratory conditions possessed somewhat less vigor than wild lines descending from the 1st to the 23d generation under laboratory conditions. During the summer (1914) food conditions were quite unfavorable. "Wild" lines descending from the 2d to the 9th generation showed a *marked* superiority in vigor as compared with the lines which during the same descended from the 96th to the 103d generation. However on the return of normally favorable food conditions the lines long reproducing parthenogenetically under laboratory conditions actually on each of the three points of comparison appeared to have a superiority of vigor.

Caswell Grave,
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(*To be continued*)